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Psychological Bulletin

A CRITICAL REVIEW OF LATENT LEARNING AND RELATED EXPERIMENTS[1]

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INTRODUCTION

Latent learning experiments have been the object of considerable interest and controversy in recent years. Careful inspection of this literature strengthens the belief that a sufficient number of experimental studies have now been accumulated to enable us to state with some specificity what conditions favor and what conditions do not favor such learning. Such a statement would aid us in interpreting apparently contradictory results, and it would facilitate the design and planning of research aiming to clarify the relationships between latent learning experiments and the various theoretical issues with which they have been associated.

The two principal theoretical issues involved are: (1) Is reinforcement a necessary condition for learning? and (2) Should a distinction be made between learning and performance? Current interest in latent learning is largely attributable to the opposite and mutually exclusive positions of reinforcement and nonreinforcement learning theorists on the first question. The former group of theorists (Thorndike, Hull, Spence, *et al.*) have postulated that certain rewarding "after-effects" or reinforcements are a necessary condition for learning. Tolman, Leeper, White, and others have argued cogently, on the other hand, that the latent learning data constitute strong evidence that reinforcement is not required for some learning. This issue is by all odds the most important one raised by this series of experiments, and it has been clearly restated by recent investigators (30, 47).

[1] The present paper is a portion of a dissertation presented to the Graduate Division of the University of California at Berkeley, in partial fulfillment of the requirements for the Ph.D. degree. The writer wishes to express his sincere appreciation to Professors Edward C. Tolman, Warner Brown, Robert W. Leeper, and Richard S. Crutchfield for generous advice and criticism.

Whereas there is agreement on the fact that one group of learning theorists regard reinforcement as necessary for learning, and another group of learning theorists do not, no similar unanimity of opinion exists as to the bearing of latent learning upon this issue. To mention but a few of the participants in this discussion, Tolman (65) and Leeper (38), as well as Seward (53), have interpreted the latent learning data as crucial in evaluating these two theoretical positions. On the other hand, Meehl (46) argues that "even the Strong Law of Effect is . . . compatible with the latent learning experiments" (p. 70). Similarly, Postman writes:

> Crucial experimental tests to arbitrate between these two interpretations of more complex forms of learning [i.e., between reinforcement and non-reinforcement interpretations] still seem to be lacking. For the time being, therefore, acceptance or rejection of the hedonistic postulate must remain a matter of philosophical preference (50, p. 501).[2]

We shall attempt to show that the results of recent experiments are by no means so inconclusive on this issue and that within the series of experiments on latent learning there are rather crucial experimental tests which enable us to reject, or at least to regard as highly implausible, one of these alternative types of interpretation.

The second issue distinguished above is less controversial, and although a number of theorists are agreed that the answer should be in the affirmative, recent statements by others suggest that the distinction between learning and performance has either not been accepted (44) or has not been found necessary (46). This distinction was first suggested by Lashley (36) and Elliott (12) and later stressed by Tolman (64) and Leeper (38). Hull's acceptance of it is embodied in his distinction between habit strength and effective reaction potential. Both Hull (27) and Spence (56) have stressed that habit strength may not be exhibited for some period in discrimination training. The issue between *these* theorists is not whether learning may occur which is not manifest in performance. Both groups admit that it may. The issue is rather one as to the cause or source of learning, i.e., whether there are any causes of learning other than reinforcement. On the other hand, McGeoch (44) has questioned the validity of the distinction, arguing in effect that such

[2] A similar scepticism concerning the testability of the law of effect has been expressed by Meehl: "So long as the Strong Law of Effect is stated qualitatively and does not explicitly mention amounts and times, it would be admittedly difficult to design an experiment in which it could be refuted" (46, p. 70). Yet, elsewhere in the same paper this writer argues that the same law *is* testable: "Such a statement (i.e., the Strong Law of Effect) *can clearly be false to fact*, either because no such trans-situational reinforcer can be shown to have been present, or because the same learning can be shown to be possible without it in the present setting" (p. 65). (Author's italics.)

a distinction is undemonstrable. And more recently, Meehl (46) has formulated the law of effect and defined "trans-situational reinforcers" so that reinforcers strengthen *responses*. Such a formulation would make difficult any distinction between learning and performance. We shall review many instances in which learning is unexpressed, or only partially expressed, in performance and which appear to justify empirically the distinction in question.

Classification of Latent Learning Experiments

Four principal types of latent learning experiments may be distinguished:

1. Type I, or the Blodgett variety of latent learning experiment, consists in giving the rats a series of unrewarded, or slightly rewarded, trials in a maze. A relevant goal object is then introduced and additional trials are given (**2, 11, 19, 48, 51, 54, 70, 73, 75**).

2. Type II is characterized by the procedure of permitting the animals to explore and live in the empty maze for brief periods of time. Subsequently a relevant goal object is introduced into the maze, and a single test trial or a series of test trials are given (**5, 9, 15, 28, 35, 38, 53**).

3. Type III consists in giving rats which are satiated for food and water a series of trials in a maze the pathways of which contain the goal objects for which the animals are satiated. After this series of trials the rats are made hungry or thirsty and given additional free trials in the maze (**30, 43, 47, 59, 62, 63**).

4. Type IV involves giving thirsty or hungry rats a series of trials in a maze containing relevant and/or irrelevant goal objects. The animals are then satiated for the formerly desired goal object and deprived of the previously undesired goal object. One or more trials are then given in the practiced maze. Many investigators have failed to find evidence of latent learning with this type of design (**13, 14, 31, 33, 41, 60, 71**), whereas positive results have been reported by others (**8, 10, 17, 38, 61, 72**).

Obviously these brief descriptions must be elaborated considerably before they can be said to state the conditions under which latent learning occurs or fails to occur. We shall shortly consider each of the above classes of latent learning experiments in greater detail. But first we must state the criteria according to which some learning in these experimental situations is said to be "latent."

Historically, the term "latent learning" has been used in two senses: (1) to refer to a learning not manifest in performance scores, and (2) to refer to a learning which occurs when the incentive is irrelevant to the existing drive (or drives). Blodgett, who coined the term and used it in the first sense, writes:

> Do these sudden drops in errors which come after the introduction of reward indicate that something to be called *latent* learning developed during the non-

reward period—a latent learning which made itself manifest after the reward had been presented? Obviously, if this is to be answered in the affirmative, one must show that the drops which occurred immediately subsequent to the reward were larger than "normal," that is, that they were bigger than drops in the control curves (2, p. 122).

Blodgett suggested two alternative criteria for determining whether such error decreases were larger than normal:

1. Greater error decreases in a non-reward group of rats upon being rewarded than any day-to-day decreases in a consistently rewarded group.
2. Greater error decreases in a non-reward group of rats upon being rewarded than in the consistently rewarded group's error decreases interpolated so that the number of errors at the beginning of the critical reductions were equal for both groups. This interpolative procedure has been described as one in which "reduction in error score for the non-reward group was compared with reduction in error score *at an equivalent ordinate* for the control" (3, p. 185).

Some investigators have questioned whether the above criteria are sufficient. Thus it has been argued that decreases in the degree of cul entries (51), or possible changes in the respiration, pulse rate, etc., of the rat (28) might provide manifest indices of the acquisitions taking place. In experiments of the Blodgett type, the question of whether there are *some* manifestations of *some* learnings during the latent period is comparatively unimportant. It is not necessary that there be no evidence of a tendency toward error reduction during the unrewarded (or slightly rewarded) trials in order for the unmanifested learning to qualify as "latent learning." The question of whether the abrupt increase in efficiency of running the maze upon the introduction of food is significantly greater than the comparable improvements of a control group is a statistical question. If it is in any given experiment, then that experiment may be interpreted as yielding positive evidence of latent learning.

Previously Spence and Lippitt (59) and Walker, Knotter, and DeValois (72) described their results in terms of the relationships of relevance or irrelevance between drives and incentives (the second of the two meanings listed above). However, the criteria according to which this relationship of "relevance" is to be determined have not yet, it seems, been explicitly stated. The question of the relevance of drives and incentives to each other is a complex one. However, knowledge of the procedures of deprivation (or experimental satiation, whichever is the case) to which the animal has been subjected and descriptions of his behavior are sufficient to determine the relationship between drives and incentives. If a rat is deprived of an object normally desired, or required for self-maintenance, and if the rat then selects pathways leading to this goal object and manifests consummatory responses toward it,

then the goal object, or incentive, is clearly *relevant* to the animal's drive (or drives). If a rat given access to an incentive until it no longer manifests consummatory responses toward the incentive, then given a series of free trials on a multiple-path maze with which it is familiar, does not select any more frequently than chance the path leading to the incentive, and, if having selected the path leading to the incentive, does not manifest consummatory responses toward it, then the incentive is clearly *irrelevant* to the animal's drive (or drives).[3] On this basis, we shall speak of "irrelevant-incentive learning," i.e., learning of the location of, and means of arriving at, an object or incentive which is irrelevant to the drive (or drives) existing at the time of the learning.

In what follows, the term "latent learning" will be used to refer to the learning involved in experiments of types I and II as classified above, and the term "irrelevant-incentive learning" will describe the learning involved in types III and IV.

Experimental Studies of Latent Learning and Irrelevant-Incentive Learning

I. *Experiments Utilizing Unrewarded Trials*

The relationships between drives and incentives may be altered either by varying the motivational state of the animal or by manipulating the incentives present in the maze situation. In the kind of experiment designated as type I it is the reward value, or incentive, which is manipulated. Simmons (54) reported the first experimental evidence suggestive of latent learning in this type of situation in 1924. Shortly thereafter, Blodgett (2) undertook a more systematic study of the phenomenon and introduced the term "latent learning." Blodgett used as a food reward a mash saturated with water; the animals were given no other water. This means that the animals were actually run under both hunger and thirst motivation. One group (the control) was fed in the food box for three minutes after each trial. A second group was treated like the first in every respect except that it was given no reward

[3] Postman, in criticizing the "irrelevant reward" experiments of Thorndike and his associates, writes: "Relevance and irrelevance are terms which refer to the *experimenter's* interpretation of the situation. . . . Relevance is a normative term reflecting the experimenter's judgment" (50, p. 515). Whereas this criticism applies to the experiments of Lorge (42) in which an *instrumental* act brought reward, it does not represent a damaging criticism of the above operational and behavioral definition of relevance. Most observers would probably grant the intersubjectivity of terms defined in this way. The concept of "irrelevant-incentive learning" should not be confused with learning by means of "irrelevant reward" (Lorge), since the latter refers to a learning which occurs *after* an originally incidental act is made relevant to the animal's drives.

at the end of the maze until the third day, while a third group was given no reward until the seventh day. Blodgett found that the animals when suddenly rewarded improved greatly in performance, their errors dropping sharply to a level comparable to that of a control group which had been rewarded from the beginning. When evaluated by the second of the two criteria listed above, these error decreases were very significant for the three-day group and bordering upon statistical significance for the seven-day group.

Herb (19) found that when the food reward was introduced into the blinds instead of at the end of the true path, her rats exhibited a significant increase in the number of cul entries after the introduction of reward. This suggests that the rats were acquiring during the unrewarded period some kind of readiness which was functional in leading to the blind alleys as well as to the goal box at the end of the true path.

Latent learning of this kind appears to occur most readily when the following favorable conditions are present:

1. *There is a complex maze or task to be mastered.* Blodgett used a six-unit T-maze. Tolman and Honzik (70) used a 14-unit T-maze and obtained, following the introduction of reward, sudden drops in error and time scores which were steeper and of greater statistical significance than Blodgett's. This difference in results may be merely a psychometrical difference arising from the use of apparatuses yielding different ranges of scores. When the test for latent learning is made by comparison of the decreases in error and time scores of experimental and control groups, as in this type of design, any increase in the sensitivity of the measurements involved should bring out more clearly any genuine differences which exist as a result of the experimental procedures.

2. *The reward introduced is highly desired by the animals or has been associated with an incentive which is highly desired.* Williams (75) demonstrated that a goal box which had been closely associated with food could, under certain conditions, function as an incentive. Halfway through the training she introduced a black and white goal box (in which her rats were accustomed to find food) at the end of the maze. Although no food was present in the box following the substitution of this box for the strange, empty one to which the rats had run during the "unrewarded" trials, there occurred a decided improvement in maze performance. Williams originally suggested that "the extent to which the learning curve drops when a reward is introduced may be taken as a measure of the value of the reward" (75, p. 33). Similarly Elliott (11) showed that the effect produced by the substitution of a new goal object was dependent upon the reward value of this object as determined by the motivational state of the animals. These experiments make it clear that such error decreases are not merely a function of the degree of "latent learning" which has occurred, but also of the value of the reward which is introduced. In view of this, it is curious that later investigators seeking to verify the Blodgett findings have utilized incentives relevant to but one of the animal's drives. It is perhaps significant that Blodgett's animals, which were both hungry and thirsty, exhibited error decreases upon being rewarded with a wet mash, while Reynolds'

rats, which were merely hungry, did not show any unusual improvement in performance when dry food pellets were introduced (51). A wet mash is probably more eagerly sought after by hungry and thirsty animals than is dry food by merely hungry animals. This interpretation would presumably also apply to the experiment of Meehl and MacCorquodale (48), in which no marked error decreases were observed upon the introduction of dry food.

3. *Prior to the introduction of reward there is present no relevant goal object or no goal object associated with a relevant incentive.* The greater the reduction in time and error scores of the experimental groups *prior* to the introduction of reward, the less it will be possible for these groups to exhibit marked error decreases. In Reynolds' repetition of Blodgett's experiment some of the experimental animals had previously been used in a spatial discrimination experiment involving a simple T-maze. Blodgett (3) has criticized this use of maze-wise animals, arguing that it was unwise to use such animals in view of Anderson's finding (1) that animals which had previously been given a series of rewarded trials on a six-unit T-maze, learned another maze under "non-reward" conditions almost as rapidly as a rewarded control group. Such results may be attributed to the association of certain maze parts, procedures, and the like with a relevant incentive. Maze-wise animals, when trained on an unpracticed multiple-unit maze, may quickly exhibit efficient maze performance even though they are trained without any known reward or under satiated conditions. With such animals it is difficult to create "non-reward" conditions, and hence latent learning is not likely to occur.

II. *Experiments Depending on Exploratory Training*

As in the preceding type of experiment it is primarily the incentive which is manipulated in type II experiments. Instead of being given a series of unrewarded trials in the empty maze, the animal is permitted to explore the maze for an extended period of time. After the exploratory phase of training the animal is either pre-fed in the goal box and then permitted to run one or more test trials in the maze (5, 38, 53), or it is placed directly in the maze and given a series of test trials (9, 15, 28).

Lashley (35) reported an experiment of this type as early as 1918. Many years later, Haney (15) repeated the experiment using, however, a complex 14-unit T-maze and permitting his animals to explore and live in the maze for 18-hour periods on four successive days. A control group was given similar handling and exploration in a simple rectangular alley path. Haney then gave both groups a series of rewarded trials in the maze. The experimental group, which had had previous exploration in the maze, made significantly fewer errors than the control group. Since the time scores of the groups did not differ significantly it is unlikely that a differential delay in reinforcement can account entirely for the observed differences in error scores.

Daub's experiment (9) provides a check on the hypothesis that the operation of doors to prevent retracing may induce some kind of mo-

tivation facilitating progress through the maze. Daub gave his experimental and control animals preliminary exploration on a 14-unit T-maze and a rectangular alley path, respectively. Subgroups of both of these groups were then given rewarded trials on the maze either with or without the use of doors. Daub found that his animals demonstrated latent learning and that the use of doors did not make any significant difference in error scores—latent learning occurred both with and without the use of doors.

An experiment using much shorter exploratory and handling periods has been reported by Karn and Porter (28). Different groups of rats were given various types of pre-training experiences, and it was observed in general that the more complete the pre-training experience, the greater the effect upon subsequent performance in the Dashiell maze. Thus, for example, one group of rats, received handling only for several days; another group had both handling and confinement in an enclosure; still another had handling plus exploratory experience in the maze, etc. The results of Karn and Porter suggest, although the differences here are not statistically significant, that unrewarded exploratory experience in the maze has greater effect on maze performance than pre-training which does not include such exploration. These results, together with those of Haney and Daub, argue against the hypothesis that the superior performance of the exploratory animals in these investigations is attributable merely to superior emotional adjustment.

Two experiments by Buxton (5) and Seward (53) employ the method of pre-feeding the animals in the goal box subsequent to exploration and just prior to the single critical test trial on the maze. In Seward's experiment it is fairly certain that the correct choice on the critical trial depended upon the preceding exploration. In one of Seward's control experiments, animals which were treated just like the original group of animals except that they were given no preliminary exploration, failed to take the correct path on the critical trial any more frequently than would be expected by chance.

In a second control experiment, Seward attempted to determine what cues his rats had relied upon, by eliminating differential cues from inside the end boxes for some animals and by eliminating cues other than intra-endbox cues for other animals. The former was accomplished by training rats in a maze in which two identically painted goal boxes were interchanged several times during the four exploration days. To check upon the effect of eliminating differential cues other than those emanating from within the end boxes, Seward gave a second subgroup its pre-feeding experience in one of the endboxes which had been moved from

its place in the maze and placed just behind the starting box. Neither of these subgroups was able to respond appropriately on the test trial, and Seward concluded that the animals in the main experiment must have reacted to cues both inside and outside the endboxes. It is possible that the rats in the latter subgroup were learning that food was to be found in the endbox in its *new location*. Seward reports that one rat on its test run tried to climb the back wall of the starting box (it will be recalled that the feeding experience had occurred just behind the starting box for this control group). This control experiment is similar to one of Leeper's (38), in which rats were fed and watered in distinctive goal boxes after a preliminary period of exploration on an elevated maze. Before the feeding and drinking stage Leeper removed the food box and water box from the maze to another part of the room. Leeper's results were also negative (although the very rapid trial-and-error learning of these rats suggests some learning during the exploration period). Care was taken in Leeper's experiment to enclose each of the endboxes so that when a rat was in any one of them it could see only the interior of that endbox. Furthermore, the rats "were introduced into the appropriate endboxes individually, being carried in such a manner that it was not possible for them to see the exterior of the boxes into which they were placed" (38, p. 25). It is unlikely that an association between eating or drinking and the new spatial locus of the boxes occurred under these conditions. Each of Leeper's three endboxes had distinctive interior textures, but rats are apparently unable to make the required associations when they must rely solely upon such intra-endbox cues during the feeding or drinking experiences. We may summarize these results as follows: If differential cues to which the animals would normally respond during the course of training are removed so that the identifiability of the endboxes is lessened, or if the cues are altered during the course of training so that conflicting expectations arise, rats are unable to solve the problem.

It is possible in any experiment which employs the kind of pre-feeding procedure used by Buxton and Seward that the correct response was mediated by a perseverating goal reaction from the endbox in which the animals have just previously been fed. Hull has already illustrated how the performance of Maier's rats might be explained in this way (25, p. 236 ff.). A third control experiment by Seward provides a test of this hypothesis. The animals in this experiment, instead of being placed in only one terminal box of the maze in the pre-feeding stage, were placed for two minutes in one endbox and then for two minutes in the other endbox. Food was available as before in only one of the endboxes,

and half of the rats were placed in the food box first and half in the empty box first. Thus, if the rats had been responding merely to a perseverating goal reaction which mediated the correct choice, then these control animals should have performed much less efficiently than the animals in the main experiment. This expectation was not confirmed; the percentage of correct choices of the controls (81.3) did not differ significantly from the percentage of correct choices (87.5) of the main group. One of Buxton's control experiments may also be cited as a check upon this interpretation. In this experiment, rats lived in and explored a 12-unit T-maze for several nights just as did the experimental groups. The pre-feeding experience was omitted for these animals, however, and they made their critical runs without finding food anywhere in or near the maze during the trials. No perseverating goal reaction from the eating of food in the endbox could have been operative under these conditions. Although the number of animals meeting the criterion of success on the first test trial was less than the corresponding numbers in the main groups receiving pre-feeding, this number was significantly greater than the number of those successful in a control group whose exploratory training was confined to a straight alley. Thus the efficient maze performance of Buxton's rats apparently depended upon preliminary exploration in the maze and does not seem to be attributable solely to any perseverating goal reaction from eating in the goal box.

Although it is now well established that rats acquire some familiarity or knowledge of a maze by exploring and living in the maze paths, it is still unknown how long the exploratory period must be in order for such learning to occur. The answer to this question will probably depend upon the complexity of the maze path to be negotiated, the number of differential cues reacted to, and many other parameters. Seward gave his animals a total exploratory period of approximately 90 minutes in a single-unit T-maze, while Lashley found that 20 minutes was sufficient to affect performance on his simple two-path maze. Workers using multiple-unit T-mazes have used total exploratory periods of 48 hours or more. Prima facie it would seem reasonable that the longer the exploratory period in the maze, the better should be the learning exhibited on the critical test trials. Since Buxton's animals explored the maze for three, four, six, or nine nights, his experiment provides some data bearing on this hypothesis. Additional amounts of exploratory training did not produce larger percentages of animals that could meet the error criterion. One explanation which Buxton offers for this fact is that individual differences in the rats may have been such that the "per cent successful" simply represented the proportion of each group capable of

mastering the task. Apparently Buxton did not give the unsuccessful animals additional trials until each met the criterion of three or less errors; at least, no supporting data are presented. It is possible that all of Buxton's experimental groups received amounts of exploratory training in excess of the optimum for these particular maze conditions, so that no gradient in clarity of knowledge (or degree of learning) was observed with increases in total exploratory time.

Another of Buxton's findings is of considerable interest. His experimental animals which exhibited latent learning also manifested a speed-of-locomotion gradient on the first run through the maze, and a backward order of elimination of blinds. Hull (23) made his deduction of goal gradient phenomena on the basis of a conditioning which is progressively weaker as the reactions of the animal are more remote in time or space from the point of reinforcement. Since the animals had never before been given a rewarded run in the maze to the point of reinforcement, the conditioning process on which Hull's deduction is based could not have occurred. Hilgard has mildly criticized Buxton's experiment on the grounds that the results reported need to be supplemented in order to interpret with confidence the gradients of running speed (21, p. 285).

To summarize, the problem with which the animal is presented in the foregoing latent learning experiments is primarily one of learning the characteristic features of the environment under non-reward conditions. It has been found that latent learning is most easily demonstrated under the following conditions: The maze environment to be mastered is complex or otherwise yields reliable measurements of individual differences in maze performance; the reward introduced is highly demanded or is associated with a highly demanded goal object; and the response by which the learning is manifested is not associated with a relevant reward during the non-reward trials. It appears, further, that if the means of eliciting an already acquired environmental familiarity is especially emphatic—as with the pre-feeding technique employed by some investigators—the learning may be demonstrated even in relatively simple maze environments. Such learning also seems to require a sufficiency of intra- and extra-maze cues and avoidance of the introduction of contradictory cues.

III. *Experiments Using Weak Irrelevant Motivation*

The alternative of varying the motivational state of the animal in order to alter relationships between drives and incentives, is illustrated in experiments of the third and fourth types (irrelevant-incentive learning). In the former (type III) the animals are trained while satiated for food and water and are presumably motivated during the training period by weak, secondary drives irrelevant to the food and water present in the goal boxes of the maze.

Szymanski's early experiment (62) was the first to utilize this kind of experimental design. Using a modified Hampton Court maze, Szymanski ran several animals when not hungry, for 61 trials through the maze to the home cage in which food had been placed. During this period there was no reliable tendency toward a reduction in time, errors, or in distance traveled per trial. During the critical trials when the rats were made hungry, however, they quickly exhibited efficient maze performance.

Suggestive as these results were, they were not followed up by experiments of similar design until recently, when Spence and Lippitt (59) initiated the Iowa series of latent learning experiments. Briefly, the Spence and Lippitt experiment demonstrated that rats which had been satiated for food and water and run on a single-unit Y-maze with food in one end of the Y and water in the other, learned the location of the goal objects. On the critical test trials, the animals which exhibited preferences for the "water side" during training were made hungry, while those showing a "food-side" preference were made thirsty. The animals made significantly correct choices on their first test run. However, on the second test run the next day, when their need was shifted, they did not make a significant shift to the correct side (R. Lippitt, personal communication).

Meehl and MacCorquodale (47) repeated the above experiment with some slight modifications in design. A single-unit T-maze was used; the terminal sections of the maze were so placed that a rat made a right turn at the end of the right alley and a left turn at the end of the left alley. During the 40 training trials under satiation, water was in one terminal section of the maze and food in the other. During the initial test trials the rats, now hungry or thirsty, responded appropriately. Although performance deteriorated (as in the Spence and Lippitt experiment) when the drive was shifted on subsequent trials, the proportion of responses to the appropriate side on all five test trials was significantly greater than chance.

The satiated group of animals in Kendler's experiment (30) received experimental training similar to the latent learning groups in the above experiments. Although this group of rats when made hungry or thirsty consistently chose the appropriate alleys more frequently than 50 per cent, Kendler did not attempt to draw any conclusions as to whether they exhibited latent learning. Since no information concerning the pre-test performance of these groups is reported, it is impossible to evaluate this finding.

On the theoretical level, perhaps the most crucial problem is that of

determining whether certain alleged secondary reinforcing agents can reasonably be interpreted as mediating irrelevant-incentive learning. This problem has been attacked by investigating the following questions: (1) *Does the irrelevant incentive itself serve as a secondary reinforcing agent?* and (2) *Is the degree of irrelevant-incentive learning exhibited a function of the delay in reinforcement of the critical S–R sequence?*

1. The evidence relating to the first question is, with one exception, negative. This is indicated by the fact that, other things being equal, rats satiated for food do not select a food alley in preference to a non-food alley. This has been demonstrated both for satiated rats (63) and for thirsty rats (13, 61, 72).[4] The exception itself is instructive from a methodological standpoint. MacCorquodale and Meehl (43) ran animals satiated for food and water in a single-unit T-maze with one goal box containing food and water and the other empty. Half of the animals found food and water on the left; the other half found it on the right. Although an attempt was made to match the animals in the strength and direction of position preferences, on the *initial* training day the group finding food and water on the left made 20 per cent more left turns than the group finding food and water on the right. This initial inequality before the animals had experienced food in the maze, detracts somewhat from the finding that there was a statistically significant difference between the two groups in the total number of left turns, with the group finding food and water on the left making the greater number of left turns. Further, these investigators employed feeding procedures which did not establish any definite, known feeding rhythm. Also, they attempted to satiate their rats for food and water simultaneously and employed a dry, "stick"-type of food. Although MacCorquodale and Meehl report that no rat ever ate or drank in the maze during the training trials, one might expect that, with a detention period longer than 30 seconds, some animals treated in this way would exhibit consummatory responses toward the incentives. A more rigorous test of the hypothesis in question would involve procedures which clearly rule out inadequate satiation due to the prior ingestion of dry food (perhaps causing thirst) or to the unknown periodicity of feeding habits. In view of these facts, and in consideration of the negative evidence referred to above, it seems reasonable to conclude that the irrelevant incentive in those experiments in which motivation is carefully controlled does not serve as a source of primary or secondary reinforcement.

2. A method of testing the second of the above questions has been suggested by Meehl and MacCorquodale (47, p. 388). If reinforcement arises from the procedures of removing the rats from the terminal sections of the maze, returning them to the home cage, etc., then the greater the distance between the irrelevant incentive (e.g., food) and this point of alleged reinforcement, the poorer should be the irrelevant-incentive learning manifested. This would follow if it is assumed that such learning is dependent upon the strengthening of connections between fractional consummatory responses (elicited by the sight of food) and the distinctive proprioceptive stimuli associated with the

[4] In an unpublished experiment (type IV) by the writer, involving approximately one hundred animals, it was also found that thirsty rats satiated for food did not tend to select a food pathway in preference to a non-food pathway.

turning responses in the terminal sections of the maze. Thistlethwaite (63) found, however, that changing the spatial location of the food did not have any particular effect upon the irrelevant-incentive learning of satiated rats, nor was the degree of such learning a function of the number of alleged reinforcements of the critical S–R sequence.

Attempts to study the relative potency of "cognition" and reinforced trials (47), or to compare the amount of learning occurring with or without (relevant) motivation (30), may be seen as different ways of obtaining data on a single question: *Does learning the location of, and means of arriving at, a given incentive occur more readily in the presence of reinforcement, or is such learning equally efficient in the absence of reinforcement?* Here the answer will depend upon a variety of parameter values. One of these appears to be the complexity of the maze path to be learned. Thus Tolman and Honzik (70), using a 14-unit T-maze, found that a group of rats rewarded only from the eleventh trial on made significantly fewer errors on subsequent trials than a control group which had been regularly rewarded from the beginning. The authors point out that the absence of a food reward during the early trials meant that the experimental group explored the maze much more thoroughly, so that when this group was later rewarded its superior familiarity with the maze permitted more efficient maze performance. Kendler, however, found that his rats learned the location of incentives in a simple T-maze more readily in the presence of relevant drives. Tolman and Honzik manipulated the incentives, whereas Kendler varied the motivational state of his animals. If the interest is chiefly in comparing learning by "reinforcement" with that by "cognition," then varying the incentive relationships is preferable to the technique used by Kendler, since the latter introduces drastic motivational differences between satiated and deprived animals and involves the Hullian assumption that food and water would allegedly act as secondary reinforcers for the satiated animals. Another parameter which must be taken into account in any critical discussion of the above question is the strength of the *irrelevant* drives operative. There is some evidence (59) that irrelevant-incentive learning occurs *less* readily when there are strong irrelevant drives present than when this is not the case. The presence of a strong thirst drive in one of the Spence and Lippitt experiments (60) to be discussed subsequently apparently had the effect of hindering learning of the food location.

IV. *Experiments Using Strong Irrelevant Motivation*

Finally, there is to be considered the variety of irrelevant-incentive experiments in which rats are trained under a strong drive. Upon the

completion of training, the animals' motivation is reversed and the test trials are given under a different drive. If thirst was the dominant motivation during training, hunger is usually the drive during the critical test runs (or, occasionally, hunger for some test trials and thirst for others). Any evidence of an appropriate modification of behavior or of a significant increase in the number of choices of that alley of the maze which contains the goal object for which the rats are now motivated, indicates irrelevant-incentive learning.

The discrepancies between various experiments of this type have not as yet been adequately explained. A critical review of these experiments casts serious doubt upon the possibility of isolating any necessary and sufficient condition for irrelevant-incentive learning under strong motivation. The reasons for this doubt will, it is hoped, become clear as we proceed. In the following sections are evaluated various experimental and theoretical attempts to isolate the crucial variables determining whether or not such learning occurs in this domain.

1. *Strength of irrelevant motivation.* In reporting the facts that irrelevant-incentive learning occurred under satiated training but did not occur when training was under a strong thirst drive, Spence and Lippitt (59) offered the tentative generalization that "latent learning does not occur in the situation where the animals 'perceived' the subsequent test goal object (food) while motivated for another goal object (water), but latent learning does occur where complete satiation made for no particular goal directedness in the experimental setup during the training series." Within a short time, however, Deisenroth and Spence (10) reported in abstract a positive case of irrelevant-incentive learning with training under thirst (of unspecified degree), and many other instances of irrelevant-incentive learning in the presence of strong motivation have since been reported (61, 68, 72).[5] The above conclusion must therefore be rejected, although it might still be maintained that such learning occurs more readily when the existing irrelevant drives are not too strong. Since experimenters working in this area have, apparently without exception, used deprivation periods ranging from 18 to 24 hours, there are no clear supporting data. However, the generalization that the stronger the drive the poorer the irrelevant-incentive learning, would seem to be a reasonable inference from the Spence and Lippitt experiments.

2. *Nature of irrelevant motivation.* The experiment of Hayes (17) lends some support to the view that such learning occurs more readily when training is carried out under hunger than when the rats are

[5] The findings of Leeper (38) have been neglected by many investigators. Priority should be assigned to Leeper, who reported an experiment suggesting positive irrelevant-incentive learning of this type as early as 1935.

trained under thirst. Hayes found that rats trained under 18 hours' water deprivation failed to respond appropriately to food in a straightaway when tested hungry, whereas rats trained under 23 hours' food deprivation responded appropriately to water when made thirsty. However, results obtained by other investigators who trained some or all of their animals under the hunger drive are at best only partially consistent with this finding. Kendler (31) found that neither the animals trained thirsty nor those trained hungry in his experiment gave evidence of irrelevant-incentive learning. The response of selecting the correct alley of a T-maze, which was required in this experiment, is qualitatively different, however, from the response of merely stopping midway down the path of a straightaway as in the Hayes experiment. Christie obtained strikingly appropriate responses on both hunger and thirst test trials with rats which had been trained under hunger motivation. However, since Christie's results were obtained with animals which had been given a variety of pre-training experiences, it is uncertain whether these results are in any way attributable to the fact that training was under hunger.

3. *Strength of preferential tendencies.* Walker, Knotter, and De Valois (72) have called attention to the relation between the strength of position preferences and the degree of latent learning manifested. They found that over 50 per cent of their "weak"-preference animals modified their performance appropriately when the motivation was changed from thirst to hunger, whereas only about 5 per cent of the "strong"-preference animals modified their performance. It is significant that in many of the experiments yielding negative results (31, 60, 71) conditions were such that the animals acquired pronounced turning preferences during the course of training. In other experiments yielding negative results, where asymmetry of reward did not cause a development of preferences, the authors unfortunately do not report what proportion of their animals exhibited strong preferences. It is therefore impossible to say to what extent the failure to find irrelevant-incentive learning in these experiments (14, 33) was attributable to overly-strong preferential tendencies. That this factor is a basic one, however, is indicated by the facts that asymmetry of reward and asymmetry of intra-maze cues appear to be associated both with the development of strong preferences and with a failure to manifest irrelevant-incentive learning. We shall now consider each of these features of experimental design in greater detail.

4. *Asymmetry of reward.* The usual experiment of this type involves a single-unit T- or Y-maze, one alley of which leads to food and the other to water. Definite preferential tendencies are acquired during the training trials because the rats which are thirsty (or hungry) quickly learn to select on their free trials that alley which leads to the appropriate incentive. One method of avoiding this outcome is to place the re-

ward which is appropriate during training in *both* endboxes (the irrelevant incentive being placed in only one alley of the maze). Experiments in which there was asymmetry of reward have generally yielded negative results (31, 60, 71). On the other hand, in one of the experiments in which positive results were obtained (72), the reward (water) was placed symmetrically with respect to the choice point. However, this condition (symmetry of reward) does not seem to be necessary for irrelevant-incentive learning in view of the fact that others (8, 10) obtained positive results without it. And it cannot be a sufficient condition in view of the negative results obtained when the reward was placed on both sides of the maze (14, 33). Further, Gleitman (13) reports that his thirsty rats did not learn the food location when no water was available during training on either side of a T-maze (a condition which might be called "symmetry of non-reward"), although these results are contradictory to those of Strange (61). From one point of view symmetry of reward might make for better learning by lessening the likelihood of strong position preferences. On the other hand, one might suppose that it would make for poorer learning by lessening the identifiability of the paths. It is probably more difficult for a rat to make a functional distinction between a water alley and a water-and-food alley than between a water alley and a food alley. In order to demonstrate this, future experiments might be carried out which would compare the irrelevant-incentive learning of animals trained satiated and run on both types of maze arrangements.

5. *Asymmetry of intra-maze cues.* One might expect that the more distinctive the cues associated with the two alternative choices, the more readily animals will learn "what leads to what." Kendler's experiment (31) sought to test this hypothesis. Kendler used a maze in which the left half of the stem and all doors, walls, and floor on the left were painted black, whereas the right half of the stem and the right alley were left unpainted. In addition there was a black curtain in the entrance to the left alley and a white curtain in the right alley. Yet no irrelevant-incentive learning was observed in the performance of Kendler's ten animals. Kendler and Mencher (33) used similar sets of differential cues and also failed to get irrelevant-incentive learning. However, it has been repeatedly observed (33, 47) that rats tend to prefer black alleys and curtains over those which are white.[6] This observed concomitant variation of preferential tendencies and differential intra-maze cues suggests the difficulty of adequately assessing the above hypothesis. The most drastic differences in intra-maze cues may have to be deliberately avoided in order to tease out the effects of dis-

[6] In a recent repetition of the Kendler and Mencher experiment, Rollins found that *every one* of his 36 rats exhibited a preference for the alley painted black (R. A. Rollins, personal communication).

tinctive, but non-disturbing, cues. What is needed, it seems, is a sampling of sets of intra-maze cues sufficiently distinctive to be differentiated easily but not so different in appeal that one set is consistently selected over the others. It is perhaps significant that Christie's (8) most appropriately responding groups were run on a maze painted a flat gray throughout, affording only differential light and spatial cues.

6. *Forced trials.* The experiment by Walker, Knotter, and DeValois (72) was designed to check upon the possibility that irrelevant-incentive learning may not occur as readily when some trials are forced. These investigators ran three groups of animals in a Y-maze with both of the terminal boxes containing water and with intermediary boxes (one of which contained food) between the choice point and endbox on each side. One group was permitted to choose freely on all 40 training trials. A second group was given 50 per cent forced trials, and a third group was given 100 per cent forced trials. A significant tendency for the animals to go to food when made hungry was demonstrated in the group receiving all free trials and in the one having 50 per cent forced trials. There did not appear to be any appreciable difference in the degree of irrelevant-incentive learning exhibited by these two groups. On the other hand, the modification in the performance of the group with 100 per cent forced trials, while in the appropriate direction, was small and did not differ significantly from chance. The effect of forcing the animal down certain paths on some of the training trials has not yet received sufficient experimental study to warrant any general conclusion. Carr (6) has summarized the results of a variety of experiments dealing with the influence of guidance, or forcing, on the learning of complex, multiple-unit mazes. As might be expected, it has been found that some mechanical guidance aids the learning of complex maze paths, and that the number of forced trials and the stage of learning at which these trials are administered are important variables determining the effectiveness of such guidance. Whether these conclusions are applicable to the problem of forced trials in the usual irrelevant-incentive experiment is uncertain. Carr and Koch (7) found that rats run on a single-unit T-maze in an alternation problem made consistently better records when all 800 trials were free then when 80 per cent of the trials were forced. It might be expected that the effects produced by forcing an animal to take the alley of a simple T-maze leading to an irrelevant incentive would be quite different according to the preferences of the animal, to the strength of the dominant drive, and to the probability of finding appropriate reward. Some investigators of irrelevant-incentive learning have reported that forcing to the non-preferred side in simple two-path mazes produced definite disturbances in their rats (31, 71).

7. *Discriminating perception of the irrelevant incentive.* Leeper has suggested (40) that certain irrelevant-incentive experiments yielded negative results because the experimental conditions were not favorable

for the perception of the undesired incentive. One of Leeper's earlier experiments (**38**) closely paralleled the Spence and Lippitt experiment, since during the first four or five days the animals were run hungry in a maze with both relevant and irrelevant incentives. It should be noted however, that Leeper's rats were trained hungry during this initial period, whereas those of Spence and Lippitt were trained thirsty. On the first trial under thirst, 13 of Leeper's 23 rats went to the now-correct side, and on the first trial of the second thirsty day, 19 of the 23 went to the now-correct side. This performance was far superior to that of the Spence and Lippitt (**60**) and Kendler (**31**) animals. Leeper observed that in his experiment, "on a wrong run, the situation was such as to guarantee that, immediately after making the run, the rat would approach the pan containing the undesired goal material and perceive discriminatingly what it contained" (**40**, p. 105). Leeper's interpretation of the negative irrelevant-incentive experiments has been vigorously criticized (**33, 58**). It is argued that the Kendler and Mencher experiment which required the animals to select water from one of five glasses (some of which contained food) satisfied this condition, and the animals nonetheless failed to learn the location of food. It is doubtful, however, whether this experiment constitutes a convincing refutation of Leeper's hypothesis. Kendler and Mencher used 50 per cent forced trials and employed a maze with clearly differentiated white and black alleys, and report that their animals exhibited a definite preference for the side of the maze painted black. Whether the position preferences acquired under these conditions were so strong that irrelevant-incentive learning was precluded cannot be determined from the results reported. Furthermore, the technique used by Kendler and Mencher of comparing *over the total 20 test trials* the performance of a group which found food on the same side as during the original thirst training with that of a group which found the food reversed to the opposite side, presupposes that learning is a gradual process of strengthening habits. On the basis of such non-continuity interpretations of learning as have been defended by Krech (**34**) and Lashley and Wade (**37**), one might predict that the reversed group would rather quickly give up a maladaptive hypothesis and learn within a very few trials to go to the now-correct side. The comparison of total performance scores over a long relearning period might well obscure, rather than enhance, any initial superiority of the constant group.

On the other hand, a comparison of Hull's results (**24**), obtained from an experiment in which rats were alternately hungry and thirsty on successive days, and Leeper's results, obtained from a similar experiment,[7] strongly suggests that it is a matter of considerable importance

[7] The comparison discussed here is between the performance of Group III-C in Leeper's experiment and that of Hull's main group. This comparison is more easily interpreted than others which might be made, because both of these groups were run in similar mazes which differed only with respect to the construction of the endbox or endboxes.

whether the animals on their wrong trials simply find a closed door at the end of the path or whether they find the incentive for which they are satiated. If Hull's curve (24, p. 263) is smoothed one might come to the conclusion that his rats reached a 70 per cent accuracy on first trials by the end of 120 days (15 eight-day periods of training). In Hull's procedure the rats were not permitted to find the irrelevant incentive on their incorrect runs. On the other hand, Leeper permitted his rats to find the irrelevant incentive on the wrong runs and found that they learned the same problem to the same level of accuracy in about 24 days (this period was estimated by smoothing Leeper's first-trial curve). Leeper concluded from his experiments that the animals "must learn almost as much from their incorrect responses as they do from their correct responses" (38, p. 37). Apparently irrelevant-incentive learning made the difference in the outcomes of these two studies. Inasmuch as an experimental setup requiring the animals to perceive the irrelevant object discriminatingly made such a vast difference in learning this problem, it seems very probable that such a setup would facilitate irrelevant-incentive learning. The Kendler and Mencher experiment and others, however, argue against its sufficiency.

8. *Discrimination of internal stimuli.* The problem which the animal is required to solve in this type of irrelevant-incentive experiment is very similar to the problem which Hull (24) gave his animals in the experiment just discussed. In both setups the problem for the rat is essentially one of learning to take one path of a maze if it is thirsty and the other path if it is hungry. There are, of course, important differences between the two problems, namely: In the Hull experiment hunger and thirst drives were alternated on successive days, and the correction method was employed so that the animals always received reinforcement on a given trial. The extremely slow learning of Hull's rats led him to suggest that there is a lack of distinctness between the hunger and thirst drives of the rat (24, p. 270). In a later formulation, Hull maintains that the problem is essentially one of learning to discriminate different patterns or combinations of stimuli (27). These stimuli are said to include the drive stimuli peculiar to hunger and thirst, distinctive proprioceptive stimuli arising from postural movements, etc. More recently, Kendler (29) has reported an experiment which casts doubt upon Hull's explanation in terms of two stimulus patterns each conditioned to a specific response. Heron (20) has also questioned, on the basis of experimental data obtained from a closely parallel experiment, whether the animals were actually making the differentiation upon the basis of drive stimuli. Heron's results suggests that his rats were using stimuli from a full versus a partially empty stomach.

Regardless of what mechanisms underlie the differentiation, however, there remains the possibility that whatever stimuli, or patterns

of stimuli, are used are not sufficiently distinctive to become quickly and clearly associated with the routes to the appropriate incentives.[8] If this is the case, it is reasonable to suppose that pre-training in making discriminations between hunger and thirst states and associating each with a distinctive response would aid the animals in making the modifications of performance required in this type of irrelevant-incentive experiment. In an unpublished study, Lippitt found that preliminary "need differentiation" exercise facilitated the differential motivational control of responses on the test trials. Lippitt's animals were satiated for food and water during training in the irrelevant-incentive experiment proper. Although the number of his animals was small, previous exercise in the discrimination of drive states and the association of each with going to one end of a straightaway, facilitated appropriate responses on the test trials, particularly on later test trials when the motivation was reversed. More recently, in an unpublished experiment by the writer, similar preliminary practice definitely facilitated learning when the training series was given under strong irrelevant motivation (thirst). Even if the conclusion is accepted that such preliminary training is efficacious, much more work will need to be done to determine how it mediates better irrelevant-incentive learning.

9. *Previous experience.* We have already observed that previous exploratory experience in the test maze itself represents one major condition under which latent learning occurs (type II experiments). Does previous exploratory exercise in non-maze environments aid subsequent performance in the maze? Several experiments suggest that it does. Hebb (18), for example, compared the problem solving of animals reared to maturity in cages with others which were allowed frequent exploration of a varied environment. The latter were reared at home as pets with much of their time spent outside their cages. Hebb reported that the group with the greater infant experience exhibited a definite superiority in various tests of problem solving.

Christie's provocative series of experiments (8) was designed to test a number of hypotheses concerning the effect of early experience in the irrelevant-incentive learning of rats. Christie subjected some of his animals to a complex regimen of water and food deprivations from the time of weaning until three months of age (for approximately 60 days). When these animals were later trained under conditions similar to Kendler's (31), Christie found that these early deprived groups exhibited irrelevant-incentive learning. One of the groups which was deprived from infancy was permitted to satisfy its needs after the

[8] Heron's technique of satiating the rats for a given incentive two hours before the day's trials probably produced more distinctive internal stimuli (i.e., more completely full versus empty stomachs) than the technique used by others of having the incentive available in the cages continually.

deprivation periods only by exploring an elevated path and table-top on which food and water were placed. The animals in this group remained there for 24 hours after the termination of each deprivation period. Since food and water were presented in close proximity to each other on the table-top, the rats were not required to associate distinctive turning responses with particular drive states. A second group which had food and water in its cages at all times was given the same exploratory exercise as the first group, i.e., they were put on the pathway leading to the table and allowed to remain there for 24 hours at a stretch. The position of the pathway was varied on successive occasions so that in no case did the animal find the path to food and water occupying the same spatial configuration on any two successive days. Christie found that both groups when later trained hungry and tested on a T-maze for irrelevant-incentive learning went to water with high degrees of accuracy on the two test trials under thirst. Moreover, the exploratory group without any previous deprivation experience was superior to the deprived exploratory group on the two hungry test runs.

In line with this are the experimental findings which Bruce (4) has referred to as "perceptual learning." Bruce gave several groups of rats ten days' practice in drinking in one square of a field which was later the goal-square, and observed that all three groups of rats that had had the practice learned more rapidly than did animals that did not have this practice. The problem which these rats had to master was to run from one end of the open field to the other end where water was available. Bruce writes regarding the animals which received previous drinking practice, "if we analyze this behavior . . . it seems possible that perceptually the animals have largely solved the problem before they have actually moved from one end of the field to the other" (p. 171).

The foregoing experiments, together with the studies of Harlow (16), have recently made an issue of the avoidance of maze-wise or task-wise subjects in learning experimentation. If it is true that the behavior of the organism is limited by the hypotheses which it brings to the task as a result of previous experience, then it must be granted that principles of learning derived only from experimentally naive subjects will be severely truncated and so much the less applicable to ordinary human or social learning. The crucial question to be asked of any latent learning or irrelevant-incentive experiment involving subjects with previous experience is whether the experience was such that the *response* which is finally taken as evidence of such learning could have been rewarded, or associated with reward, as a result of this previous experience. In Reynolds' experiment (51), which has been criticized for the use of maze-wise animals, it is not clear whether the response of running the six-unit T-maze could have been associated with reward as a result of the previous maze experience. On the other hand, in the above experiments the correct response on the later test trials definitely does

not appear to have been mediated by any peripheral responses which became strengthened as a result of the previous training.

10. *Amount of training.* Spence and Lippitt (60) have pointed out that their thirsty rats learned to go with almost 100 per cent accuracy to the water alley of a Y-maze after having had an average of only seven to eight experiences with this goal object. They have argued that *if* thirsty rats (satiated for food) can learn which path leads to food without reinforcement, then their rats should have done so after having had the number of experiences of food which their experiment provided (an average of 26 trials to the food alley). Similarly Walker, Knotter, and DeValois (72) have inferred that a response acquired under an irrelevant drive is much weaker than one acquired under a relevant drive. This conclusion is based on the observation that a training series of 40 trials under thirst motivation was adequate for rats to acquire a water-going response overcoming even the strongest position preferences (71), whereas the food-going response demonstrated when rats were made hungry after a similar period of training was not strong enough to overcome the strongest position preferences (72). Such comparisons, however, only indirectly bear upon the question of the relative efficiency of learning with reinforcement as compared with learning in the absence of reinforcement. The problem which the rat must master in this type of experiment is not simply that of going to one end of a Y-maze for food. As was pointed out above, the problem is essentially one of learning to take one path of the maze when thirsty and the other path when hungry. Hull's experiment (24), checked and supported by Leeper's experiment III-A (38), provides us with relevant data as to how rapidly rats can master this problem when every trial leads only to reinforcement and not to perception of the irrelevant incentive on the wrong runs. Smoothing Hull's curve, we may estimate that his rats when run under the correction method required approximately six hundred trials to attain a 70 per cent level of accuracy. As we have already seen, under the more favorable conditions in Leeper's experiment III-C, rats required approximately 24 days of training, or about 120 trials, to reach and maintain a 70 per cent level of accuracy on first trials (based on a smoothing of Leeper's curve).

It is true that most irrelevant incentive experiments have employed the non-correction method and that some of them have been designed to facilitate perception of the irrelevant incentive on the wrong runs. However, one might expect that investigators seeking to determine whether rats run under a strong irrelevant drive can learn to master this problem without reinforcement would give the animals, at least, a similarly large number of training trials. Actually we find that the number of training trials in the relevant studies have ranged from 12 to 60. It should be obvious that no irrelevant-incentive experiment involving a training period falling in this range can provide a convincing

demonstration that rats, run under a strong irrelevant drive, typically do not learn the problem in the absence of reinforcement. On the other hand, the fact that some investigators (8, 72) have obtained positive results with relatively short training periods indicates that when conditions are especially favorable smaller amounts of training may suffice. It is possible that previous exploratory exercise, or preliminary training in the discrimination of drive states, serves as a substitute for otherwise inadequate amounts of training. The Leeper and Hull experiments, it should be pointed out, were so designed that both pathways were "correct" an approximately equal number of times during the training series. On the basis of these experiments we can infer that longer periods of training under conditions which do *not* produce any strong asymmetrical tendencies will probably facilitate irrelevant-incentive learning.

Summarizing, it appears that the problem with which the animal is presented in the foregoing studies of irrelevant-incentive learning is essentially one of learning relationships between characteristic features of the environment and various drive states. Irrelevant-incentive learning seems to occur most readily under the following conditions: weak irrelevant motivation, hunger rather than thirst as the irrelevant drive, weak position preferences, symmetry of reward during training, free rather than forced trials, discriminating contact with the undesired goal object, previous exploratory or "need" differentiation exercise, and large amounts of training under conditions which do not produce strong asymmetrical preferences.

Theoretical Interpretations

Some of the above experimental studies provide a rather conclusive answer to the first question raised in this survey: Is reinforcement a necessary condition for learning? Since the sources of reinforcement are pervasive and often subtle in nature, a careful examination of the evidence and its relationship to such theories is necessary. Unfortunately, limitations of space forbid anything more than the barest outlines of the various deductions which might be made from reinforcement theories of learning.

If the indispensability of reinforcement for learning is to be granted, it must be possible to demonstrate for *each* instance of latent learning or of irrelevant-incentive learning (1) that some source of reinforcement was operative in the experimental setup and (2) that the changes in responses which are taken as evidence of the learning can be deduced on the basis of this alleged reinforcement. Much of the equivocality of "crucial" tests in this area is attributable to the failure of theorists to state the conditions under which certain alleged sources of reinforcement are present and active. In the strict sense, as long as such statements are lacking, reinforcement interpretations of latent learning or

irrelevant-incentive learning are unverifiable. However, if the second of the above requirements can be satisfied, then the set of reinforcement postulates in question may have some degree of plausibility, this unverifiability notwithstanding. The evidence, however, is of such a nature that it appears to be impossible to deduce, from any existing reinforcement theory, the behavioral changes indicating latent learning or irrelevant-incentive learning in each of the reported experiments. One intractable instance alone would suffice to answer our question. Actually, there are many examples of such learning which are difficult, if not impossible, to deduce from present reinforcement postulates.

1. Several instances (**2**, **19**, **70**, **73**) of type I experiments are difficult to explain in reinforcement terms. Possible sources of reinforcement in the Blodgett variety of latent learning, which immediately come to mind, are satisfactions of curiosity and escape from the maze. Conjectures based upon such alleged reinforcements have already been critically evaluated by Leeper (**39**, pp. 27–28) and by Hilgard (**21**, p. 284), hence we need not repeat these considerations here. The chief shortcoming of such conjectures is that the abrupt behavioral changes following the introduction of reward simply have not been shown to be deducible from the reinforcement postulate. More recently, Meehl (**46**) has suggested that such abrupt behavioral changes might be deduced by means of an additional assumption regarding the asymptotes of the habit growth functions involved. Meehl writes:

> A neo-Hullian . . . might assume that when a response undergoes an increment in strength on the basis of a minimally reinforcing agent (that is, one in which the asymptote of the acquisition of habit strength is relatively low), then, if subsequently a strong reinforcement is introduced, the parameter in the new growth function which determines the rate of approach to the new asymptote is greater than it would have been without the original learning. Since in the Blodgett design there is evidence of acquisition of differential habit strengths during the latent phase, such a postulate would lead to a preservation of the Strong Law of Effect (**46**, pp. 70–71).

There are considerations which make this explanation implausible. First, it is assumed that there must be an acquisition of differential habit strengths during the latent period, or that the finally-to-be-strengthened receptor-effector connections are being strengthened during the latent period. However, the experiment of Herb (**19**) demonstrated that rats when properly motivated could perform responses *inimical* to those which were, by this interpretation, slightly rewarded. It will be recalled that Herb's rats exhibited an ability to enter the culs when food was placed there, even though the effect of the learning which presumably occurred under the "minimally reinforcing" conditions should have been the formation of connections so that stimuli from

the maze should have evoked reactions equivalent to more or less entering upon and running the true path. Similarly, the experiment of Wallace, Blackwell, and Jenkins (73) showed that latent learning occurred even when the rats were removed at chance points in the maze and even when the goal box was absent so that the crucial response could not have been practiced and reinforced. This procedure seems to rule out clearly the differential reinforcement of the finally-to-be-rewarded response.

2. In those instances (5, 9, 15, 53) of latent learning which depend upon previous exploratory training (type II), satisfaction of curiosity and escape from the maze again come to mind as possible sources of reinforcement. Buxton (5, pp. 4–7) has critically evaluated the former possibility, and Leeper's criticism (39) is again applicable. In short, the difficulty here is that the alleged exploration-reinforcement occurs in the presence of "incorrect" as well as "correct" connections and offers no basis for the differential conditioning of correct responses. In the Daub and Haney experiments, the rats were presumably removed from the maze at chance points. Buxton's rats were introduced into, and removed from, the maze at various positions which were systematically varied so that no parts of the maze were favored or handicapped, in learning, by this treatment. Similarly, Seward's procedure of removing his rats from a simple T-maze does not appear to involve any differential strengthening of right or left turns. Escape-reinforcement, therefore, does not appear to provide the means by which such learning can be deduced. A third type of explanation is possible for those experiments in which rats are fed in the goal box immediately prior to the critical test runs (5, 53). It might be argued that the correct response in such experiments is "essentially a case of the perseveration of a goal reaction . . . from one stimulus situation to a closely following one . . . " (25, p. 243). However, the control experiments by both Buxton and Seward demonstrate rather clearly that their results cannot be entirely accounted for on such a basis. Finally, the application of Hull's concept of anticipatory goal reactions, in a manner analogous to his analysis of Maier's four-pathway reasoning experiment (25, p. 221 ff.), does not appear to be possible here because all of the habit segments involved in the finally correct response are not securely anchored to any common pure-stimulus or symbolic act. On the other hand, Seward's theoretical derivation (52, 53) of latent learning in such types of experiments demonstrates that such learning, while not readily reducible to reinforcement terms, may be interpreted with some adequacy in S–R terms. However, Seward's construct of the "surrogate response" is essentially central in character; hence his analysis is not to be construed as one in terms of the association between peripheral stimuli and reactions.

3. Positive evidences of irrelevant-incentive learning in type III

experiments (47, 59, 63) similarly require explanation. Meehl and MacCorquodale have attempted to provide a reinforcement interpretation of the learning exhibited by their satiated animals. Their derivation assumes distinctive turning responses (at each end of the maze arms) which become conditioned to fractional consummatory responses elicited by the sight, smell, and touch of the irrelevant incentives. Hull (25) and Miller (49) have similarly stressed that special devices calculated to induce very distinctive postures and goal reactions should be introduced before fractional anticipatory goal reactions can be expected to play a directive role in the utilization of habits. Meehl and MacCorquodale have suggested that one method of testing their assumption would be to reverse the turns required at the ends of the maze arms. They have predicted that the stimuli associated with the "wrong" turning responses would then become associated with fractional eating and drinking responses, and the animals, when made hungry or thirsty, would fail to go to the appropriate alley and very probably go to the inappropriate alley. Maltzman (45) has presented a closely analogous derivation which relies upon the conditioning of fractional anticipatory responses and hypothetical proprioceptive stimuli associated with choice-point turns.

As yet the only part of the above derivations to receive experimental study has been the set of assumptions regarding the relationships between habit strength and various aspects of the alleged reinforcements. Since irrelevant-incentive learning does not appear to be related to any *demonstrable* secondary reinforcing effect from the sight, smell, and touch of food, nor to the degree of delay in escape or social reinforcement, nor to the number of alleged reinforcements of the critical S–R sequence (63), it may tentatively be concluded that such learning is not dependent upon reinforcement.

4. Lastly, certain instances of irrelevant-incentive learning (8, 61, 72) in type IV experiments may be considered for their compatibility with reinforcement principles. A reinforcement derivation similar to the foregoing derivations has been presented by Spence.[9] For the situation in which food is in one goal box of the T-maze and water is in the other, the derivation would presumably assume that rats trained thirsty obtain primary reinforcement in the water alley and secondary reinforcement in the food alley. Elsewhere, Spence has stated that the sight and smell of food will have the capacity to elicit the reinforcing mechanism (55) and that such secondary reinforcement regularly plays an important role in maze learning (57). The stimuli in the food

[9] Unpublished paper reported at the 1941 meeting of the Midwestern Psychological Association; results presented in abstract (10). Kendler (29, 32) has reported summaries of Spence's derivation. Several previous papers provide the basis for this use of the construct of the fractional anticipatory goal response, especially those of Hull (22, 25, 26) and Miller (49).

box and in the alley leading to the food box are assumed to be associated with anticipatory eating responses. The characteristic proprioceptive stimuli resulting from these goal responses are themselves conditioned to entering the alley leading to the food box. When the animals are made hungry on the critical test trials, the proprioceptive stimuli associated with anticipatory eating responses elicit the response of entering the food alley. This derivation is not supported, however, by the finding that, other things being equal, non-hungry rats do not tend to select a food alley in preference to a non-food alley, as might be predicted on the basis of one interpretation of the concept of secondary reinforcement. Furthermore, the predicted outcome is inconsistent with the negative results of a number of other irrelevant-incentive experiments (31, 60). A similar application of the above assumptions could presumably be made for the situation in which there is symmetry of reward (72) or symmetry of non-reward (61) during training. When the animals are made hungry and fractional anticipatory eating responses are elicited by the hunger drive, the response tendency to the food alley should be greater than that to the non-food alley. But again, if such a derivation is accepted, the reinforcement theorist then faces the same problem as Tolman—viz., that of accounting for the negative results often obtained with this type of design.

Perhaps for this reason, Spence has published (60) not his original deduction but a different one, which predicts no learning of the location of the irrelevant incentive. This deduction neglects the alleged secondary reinforcing effect from the sight, smell, and touch of the irrelevant incentive on the wrong runs. If this is taken as the correct deduction and the one which Hull and Spence now wish to regard as implicit in the Hullian set of postulates, then additional assumptions will have to be made to account for the above instances of irrelevant-incentive learning. As yet reinforcement theorists have not attempted to provide the rationale by which selection of the appropriate set of assumptions is to be made.

Negative irrelevant-incentive experiments in which training is given under a strong drive (14, 31, 33, 60) have sometimes been represented exclusively as "experimental tests of Tolman's sign-gestalt theory of learning. Such tests are, of course, equally critical of the original Spence deduction made upon the basis of strict reinforcement analysis, and consequently these tests cannot be regarded as crucial between the interpretations in question. In recent years, Tolman has suggested a number of specifications of the sign-gestalt theory which render it more compatible with the above results.

1. The presence of "too strongly motivational or of too strongly frustrating conditions" is said to be one circumstance under which narrow, strip maps rather than broad, comprehensive maps seem to be induced (66). Thus ac-

cording to this specification, irrelevant-incentive learning under strong motivation (type IV) may be expected to be poorer in general than under weak motivation (type III). Similarly, Tolman has pointed out that the use of doors (for the purpose of forcing the rats down a particular pathway) in an experiment of the Spence-Lippitt type may produce disrupting conflict, or frustrating situations (67).

2. Tolman has recently stated (69) that the ability to discriminate drives may have to be learned and that this learning may represent a different type of learning from that which is involved in the acquisition of field expectancies.

3. Further, we may note Tolman's emphasis (68) upon the importance of "perceptual readiness" factors as determiners of whether hungry rats will acquire field-expectancies of water-locations. Although Tolman has not as yet indicated any operational definitions of such factors, he has pointed to the learning of Christie's exploratory rats (8) as one illustration of their operation.

4. An alternative way of conceiving the same empirical relationships which the above hypotheses seek to explain is to be found in Tolman's assertion (67) that "even though a rat's drive be changed from thirst to hunger, his need-push may not, for a while, change correspondingly." In this connection it should be added that Tolman recognizes different goal objects may have different "valences" (67). It is possible that the type of dry food pellets commonly used in such irrelevant-incentive experiments do not induce strong valences in rats. Young has recently called attention to the importance of palatability as a variable in food-seeking behavior. He writes: "One reason why the quantitative psychology of food-seeking motivation has been so backward and inexact lies in the fact that the strength of hunger and thirst has been controlled solely through deprivation, palatability relations being ignored" (76, p. 109).

It may be objected that many of the above specifications are a posteriori and that any one of them is little more than a duplicate description of the behavior it seeks to explain. There is no reason methodologically why theorists should not modify their formulations so as to be in closer agreement with empirical findings. What is objectionable, from a methodological standpoint, is the introduction of assumptions and postulates designed to explain *merely* this or that particular phenomenon, i.e., ad hoc explanations. The above specifications do not appear to be of this kind but are stated in general terms, and their applications in quite different contexts (for example, to social learning and to typical clinical phenomena) are suggested.

Conclusions

A survey of latent learning experiments over the last three decades seems to justify the following conclusions:

1. A number of latent learning and irrelevant-incentive learning experiments constitute exceptions to the alleged indispensability of reinforcement in the learning process.

2. Such unreinforced learning has been exhibited in a variety of experimental situations and confirmed by studies sampling different conditions within each of several differentiated classes. Certain conditions are favorable to such learning, and other conditions are unfavorable. These conditions have been tentatively identified within certain broad limits: some appear to be predominantly motivational and perceptual in character; others have to do with controlling the development of preferential tendencies; and still other conditions seem to have only a psychometric or mensurational significance.

Much of the difficulty of interpreting the irrelevant-incentive experiments reported to date seems to be directly traceable to the fact that no single investigator can adequately sample a variety of experimental conditions, while uncoordinated work by different investigators seemingly leads to the collection of many sets of data which are not comparable and which therefore can be assessed only with considerable uncertainty. Since the study of latent learning and irrelevant-incentive learning has always been, at least implicitly, a study of the motivational and perceptual conditions which affect learning, it may be expected that many of the questions raised by this series of studies will occupy an important role in future learning experimentation. It is proposed that future research in this area be accompanied by a greater degree of cooperative planning among the interested investigators, using if possible mutually acceptable or standard experimental designs.

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EXPERIMENTAL EVIDENCE FOR THE MOTIVATING FUNCTION OF REWARD

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INTRODUCTION

In a previous article (**33**) I proposed a theory to account for the action of reinforcers in a learning situation. According to that theory a reinforcer does not strengthen a habit as such but provides motivation for its use on future occasions. In this respect my position is like Tolman's; it may be considered an attempt to work out in more detail the mechanisms implied in such a view. Briefly, I suggest that an animal in a state of need is motivated not only by a primary drive (D) and drive stimulus (S_D) but by a secondary drive consisting of a set (rs_G) to make a characteristic consummatory or goal response (R_G). When a response (R) is followed by reward, R_G is conditioned to concurrent stimuli. By generalization of this conditioning, stimuli accompanying R now serve to intensify rs_G. It is further proposed that this intensification be called *tertiary motivation* and that it be endowed with the property of facilitating R, the activity in progress.

Our theory holds that a useful distinction may be drawn between two classes of learned responses, habits ($_sH_R$) and motives ($_sH_{rs_G}$). Both classes are covered by a general law of association of which Hull's Postulate 4 (**18**, p. 178) is a special case referring to $_sH_{rs_G}$. Reaction potential ($_sE_R$) is conceived of as a product of the following factors (in addition to stimulus intensity dynamism (**19**): D, $_sH_{rs_G}$, and $_sH_R$. This brief résumé is enough to indicate that, although the content of the theory stems from Tolman, its form is taken almost bodily from Hull.

The aim of the present article is to test the above formulation in the light of experimental evidence. To this end I have chosen, as particularly relevant, evidence as to the role of the incentive in learning situations. Specifically we shall consider experiments on the effects of the following factors: (1) properties of reward, such as amount and palatability; (2) changes in properties of reward; and (3) introduction of reward. One highly significant operation—removal of reward—must be omitted until our theory has been extended to include the facts of experimental extinction.

PROPERTIES OF REWARD

Simple Locomotor Response

Amount of reward. A number of studies (**5, 10, 39, 43**) have shown that animals will approach a large reward more readily than a small

one. Only two of these were sufficiently systematic and extensive to permit a test of Hull's quantitative statement (20), here adopted, that ${}_sE_R$ at any stage of learning is a positive exponential function of the square root of the weight of food reward ($\sqrt{w}$). Crespi (5) measured the mean running speed of different groups of rats down a 20-foot alley to varying amounts of food. Zeaman (43) also varied the amount of reward but measured the latent time of starting down a three-foot elevated pathway. In both studies increase of incentive was accompanied by improved performance. Moreover, if measures taken after improvement has ceased are plotted against $\sqrt{w}$, a curve suggestive of an exponential function is obtained.

Crespi was not primarily concerned with learning, though the three curves of improvement he presents seem by inspection to approach their asymptotes at the same rate. Zeaman, however, made a direct test of Hull's assumption that this rate of approach is independent of the incentive function. On fitting exponential equations to the mean log latencies during acquisition he showed that the constant controlling fractional increment per trial was practically constant. As Zeaman reminds us, these findings can be used to confirm Hull's hypothesis only by assuming that ${}_sE_R$ is proportional to performance measures; i.e., to running speed as measured by Crespi and inversely to log latency as measured by Zeaman. The latter assumption is not made by Hull. He makes ${}_sE_R$ a power function of latency (9, 18, 19), thereby implying that a linear relation to log latency could be approximated only over a limited range. But if Hull's mathematical postulates fail of exact confirmation, they are confirmed at the level of approximation which is more characteristic of his system in its present state (see 9, p. 525, footnote 8).

So far, Hull's revised postulate set appears adequate to account for the data. But Wolfe and Kaplon's (39) most striking discovery remains unexplained. They compared the performances of chicks as affected by one of three incentives: one large piece of popcorn (Group I), one quarter of a piece (Group II), or four quarters (Group III). Since the largest differences they found, on the whole, were between Group III and the other two groups, they concluded that amount of consummative activity was more effective than amount of reward.

The theory here proposed attributes the influence of an incentive to the arousal of a fractional antedating goal reaction, r_{S_G}. The more intensely r_{S_G} is aroused the greater will be its effect. At first glance, therefore, it seems reasonable to expect that four R_Gs in rapid succession will be more effective than one. Closer inspection reveals two ways in which such a result may be obtained: (1) It is possible that each R_G may function as if it were a separate trial and add an increment of habit strength to ${}_sH_{r_{S_G}}$; (2) the successive R_Gs on a single trial may summate to condition a more vigorous r_{S_G} to the situation. That is,

repeating R_G may either condition rs_G more rapidly or condition a stronger rs_G. The two possibilities are open to experimental test. If Wolfe and Kaplon's finding were due to accelerated conditioning the performance curves of Groups I and III should approach the same limit but at different rates. If it was due to an intensified R_G the curves should approach different asymptotes, but their relative slopes would be the same. The data at hand do not permit a choice between these alternatives; the second one, however, seems more likely.

Either Hull's theory or the present revision can handle this interpretation quite readily. Hull's "incentive function" (**20**) need only be modified by substituting for $\sqrt{w}$ the construct R_G, thus:

$$K = 1 - 10^{-kR_G},$$

in which R_G stands for the intensity of the consummatory response. R_G in turn may be considered a function of several characteristics of the goal object; e.g., weight, size, nutritive value, palatability as measured in preference tests, and amount of consummative activity. The more precise form of this function must await the outcome of experiments in which these determinants are systematically varied.

Palatability of reward. Study of one of these dimensions, palatability, has already been carried some distance by Young (**42**). He defined palatability as "the relative acceptance of food [as] dependent upon the characteristics of the foodstuff" (**42**, p. 100). Within the general problem of how this property is related to behavior, Young asked himself the specific question: Does it influence learning or performance? In one experiment (**40**) he trained rats to run to sugar, wheat, or casein in counter-balanced order and found consistent differences in rate of running to the three incentives in successive cycles. His method of separating learning from performance was to equate the total times for a series of 30 runs and compare the numbers of runs in successive thirds of that total. The data for all three incentives, when treated in this way, turned out to be closely similar. Young concluded that although palatability was a factor in rate of running there was no evidence that it determined rate of learning.

Two features of Young's method limit the generality of his finding. (1) His rats had to shuttle back and forth between starting box and food; it is possible that if they had run one way only the results might have been different. In another connection Lawrence and Miller (**24**) showed how a similar change of technique might lead to an opposite conclusion. (2) His analysis was based on the first 30 runs to each incentive rather than on the total time required to reach an asymptote. His conclusion may therefore apply only to an early stage of practice. As far as they go, however, Young's data on palatability agree with those on amount in strengthening the view that rewards function mainly as motivators rather than "associators."

Spatial Discrimination

In one of Grindley's (10) experiments his chicks had to find their way to food through a simple maze and did so slightly faster to six grains of rice than to one. But no "error" (excess distance) scores were given. Similarly Wolfe and Kaplon (39) included a single-unit T-maze in their study; they reported a smaller error total for the group running to a multiple incentive than for either of the others. An experiment aimed directly at this problem was recently carried out by Reynolds (30), who compared two groups of rats in learning a single-unit T-maze to a 30-mg. pellet and a 160-mg. pellet, respectively. The large reward group proved consistently superior both in per cent correct choices and in mean running times. Young (41) blocked the preferred path in a Y-maze and compared both time and excess distance scores of rats running to sugar and to casein. His results are not strictly comparable with those of Reynolds, who used a non-correction method, but they point to the same general conclusion: that the increase in preference for a food path over a non-food path is a function both of amount and of palatability of reward.

Visual Discrimination

In another experiment, however, Reynolds (29) trained two groups of rats on a black-white discrimination problem, again using a 30-mg. and a 160-mg. pellet, respectively; but here he found no appreciable difference between them either in mean number of trials to a criterion or in per cent correct responses during learning.[1] Similar results for palatability were obtained by Young (41), who compared the rates of learning a black-white discrimination to sugar and to casein as rewards and found no difference in error scores. How can these negative findings be reconciled with the positive ones on locomotion and spatial discrimination?

Young's conclusion from his own results is much like the writer's. He held that the law of effect applies not to "ordinary" learning but to the acquisition of a food-seeking drive. As to *how* it applies he wrote: "Degree of effect is related not directly to the rate of acquisition but rather to the strength of drive which is acquired" (42, p. 300).

Reynolds made a highly similar interpretation but stated it more precisely; we shall therefore consider it here in some detail. It will be recalled that Hull (18), on the basis of Gantt's (8) and Grindley's (10) data, had made the limit of habit strength an exponential function of

[1] This experiment is open to some question on grounds of method. As the author points out, the low-reward group may possibly have been favored by an unequal elimination of slow learners and by use of a correction method, permitting differential reinforcement of wrong responses. We are here assuming that his results can be generalized beyond these conditions.

amount of reward. To account for his negative finding Reynolds proposed that this equation be "limited to responses which can be considered replications of the final consummatory response" (29, p. 768). According to his "R-hypothesis" Hull's equation simply means that different amounts of reward arouse consummatory responses of different degrees of strength to be conditioned. Obviously this suggestion, too, has much in common with mine. I, too, propose that Hull's Postulate 4 be restricted to the conditioning of the goal response or a fraction thereof and that his "incentive function" be taken to represent the strength of that response.[2]

In its present form, however, there is room for doubt that the R-hypothesis leads to unequivocal predictions. The trouble seems to lie with the definition of terms. Reynolds defines R_G as "the complete pattern of activity commonly associated with the appearance of the reinforcing stimulus" (30, p. 152). He then goes on to define a learned replicate response as "one which can be measured on the same dimensions as the consummatory response." It is doubtful whether these definitions provide adequate criteria for distinguishing replicate from non-replicate responses. What about a bar-pressing response, for example? Learning a spatial discrimination presents similar difficulties. If running down a straightaway is a replication of R_G, then why not running down an alley with a right-angle turn in it? A T-maze thus presents a choice of two replicate responses; since one of them is reinforced it will be stronger than the other. But a large reward will increase this difference more than a small one, so the correct turn will be earlier and more consistently chosen. So far so good. But if, by the R-hypothesis, spatial discrimination is a function of amount of reward, the same prediction should apply to visual discrimination. Here the problem may be thought of as learning two replicate responses, one to white, the other to black; the rest of the argument is essentially the same.[3] Of course these interpretations do not *have* to be made; my point is that as long as they *can* be made the hypothesis runs the danger of "predicting" whatever results are obtained.

It should be clear that this criticism is directed at the definitions, not at their author's use of them. In practice Reynolds has confined

[2] Postulate 4 was confusing in that ${}_sH_R$ was actually a complex entity determined partly by strength of response and partly by strength of conditioning. In his revised postulate set (20) Hull has removed this source of confusion by defining ${}_sH_R$ strictly in terms of number of reinforcements and entering the determinants of *response* strength (amount and delay of reinforcement) separately in the equation for ${}_sE_R$. Since my proposed revision retains the symbol H in its original capacity, the same purpose can be served by introducing a new symbol, such as ${}_sC_R$, to denote strength of conditioning.

[3] Only a sketch is suggested here. A complete proof would have to take account of other factors; e.g., generalization and extinction in the non-correction method, delay of reward in the correction method.

the term "replicate response" to measures of locomotor speed. He himself derived his T-maze results not directly from the R-hypothesis but indirectly from the shorter delay of reinforcement produced by the larger reward. The question remains, however, why he did not make the same prediction for discriminating white from black, especially since his large-pellet group gave shorter response times throughout.

To criticize is one thing; to suggest anything better is another. Before giving a precise meaning to R_G I should like to know what particular reaction, if any, is essential to "reinforcement," or tertiary motivation. Meanwhile I believe the term will be most useful if we confine it to actual commerce with the goal object; e.g., to eating. This usage will enable R_G and other habits, locomotor or otherwise, to vary separately. The effect of increased reward on any habit R will then depend on how it affects R in relation to other responses evoked by the situation. We are thus in a better position to reconcile the divergent findings on spatial and visual discrimination.[4]

The learning of a spatial discrimination has already been dealt with in terms of the present theory (**31**, **32**). Essentially it involves the building up of a stronger reaction potential to stimuli accompanying a right turn, say, than to those accompanying a left. That is, ${}_{s_R}E_{R_R} > {}_{s_L}E_{R_L}$, which in turn depends on the relation: ${}_{s_R}H_{r_{s_G}} > {}_{s_L}H_{r_{s_G}}$. If by increasing the intensity of r_{s_G} the strength of ${}_{s}H_{r_{s_G}}$ is doubled, the two competing ${}_{s}E_{R}$s and the absolute difference between them are also doubled. If we grant certain other assumptions this will result in a higher proportion of correct choices.

Applied to discriminations other than spatial, however, the above account is seen to be inadequate. Lawrence's recent evidence (**23**) points strongly to the existence of a "mediating process" in such discriminations, which apparently corresponds to some sort of perceptual organization. His results imply what field theorists have long maintained: that an essential problem for the animal is learning to which cues to respond. Reynolds' rats, for example, had not only to associate white with food but to *perceive* white in a sense that goes beyond merely seeing it. Or, to use a convenient notation, in order to acquire ${}_{s_W}H_{r_{s_G}}$ it was also necessary to acquire ${}_{s}H_{r_{s_W}}$.

In maze learning this mediating, perceptual process, though presumably present, has usually escaped attention. The reason is that rats generally respond to the important spatial cues, external and internal, from the beginning. But studies of place vs. response learning (**2**, **3**, **38**), by setting external and internal cues against each other,

[4] The following discussion is aimed primarily at Reynolds' study. Young's method differed in several respects—low level of hunger, 24-hour spacing of trials, separation of cues from reward, scoring of time and errors—which may call for a different explanation.

have brought this aspect into prominence. What is learned in a maze with food on the right, then, could be more accurately designated: ${}_{S}H_{r_{S_R}}H_{r_{S_G}}$.

Let us now return to B–W discrimination. If we assume, with Spence (34), that a rat starts with a position preference, say to the right, it is quite likely that after a few trials we shall still have the inequalities: ${}_{S}H_{r_{S_R}}H_{r_{S_G}} > {}_{S}H_{r_{S_L}}H_{r_{S_G}}$, and ${}_{S_R}E_{R_R} > {}_{S_L}E_{R_L}$. But this is precisely the condition on which we have seen that learning a *spatial* discrimination depends. Furthermore, the greater the reward the greater will be the difference in ${}_{S}E_{R}$, and the greater this difference the more quickly will the animal make his choice. One more assumption is that the less time an animal delays in the choice area, the less chance he will have to respond to S_W or S_B rather than to S_R or S_L. From this argument the conclusion follows that an increase of reward will *interfere* with the learning of ${}_{S}H_{r_{S_W}}$.

It may be objected that I have overshot the mark, since it looks as if Reynolds' low-reward group should have been superior, whereas actually the means of the two groups were about the same. It must be remembered, however, that the deterrent effect of a large reward applies only to the acquisition of ${}_{S}H_{r_{S_W}}$; its effect on the other half of the process, ${}_{r_{S_W}}H_{r_{S_G}}$, will be just the opposite. Moreover, some rats in both groups may have weak position preferences and respond to S_W early in practice; of these the ones in the high-reward group should learn most rapidly of all. On the empirical side two characteristics of Reynolds' data may be cited in support of this interpretation: (1) the shorter mean response times for the high-reward group; (2) the greater variation and skewness of its learning score distribution (on the assumption that the tail of the distribution is contributed by a few rats with extremely persistent position habits).[5]

Changes in Properties of Reward

Amount of reward. Besides comparing animals given different amounts of reward, several of the above investigators studied the effects of varying the amount of reward given the same animal. Wolfe and Kaplon, for example, at the close of their runway experiment gave the group trained to one small piece of popcorn two extra sessions with four pieces. "Their time scores immediately approximated those of group three" (previously trained to four pieces) (39, p. 358). Crespi and Zeaman measured the effect of suddenly increasing or decreasing amount of reward after a group had reached its asymptote and reported not only an abrupt change in level of performance but a "contrast effect." Crespi found that rats shifted to 16 units either upward or downward,

[5] Actually the significance of these statistics for the point at issue is based on the assumption that there is a positive interaction between strength of initial position preference and the greater reinforcing effect of a large as compared with a small pellet.

overshot the mark as compared with a group run to 16 units from the beginning. Zeaman found the same effect, but in his experiment it was significant only for a relatively large shift and then only in the upward direction.[6]

Two features of the contrast effect require explanation: (1) the abruptness with which the new level was reached, and (2) its discrepancy from that of the control group (5, Figs. 7 and 8; 43, Figs. 2 and 5). As previously pointed out (33), Hull's 1943 postulate set provided for neither of these phenomena. His 1949 revision (20), we noted, allows for rapid changes in ${}_{S}E_{R}$ with changes in K, the incentive function, but does not account for the learning of K; neither does it predict contrast. Crespi's own theory, that incentive-value, as one factor in motivation, is proportional to the distance between levels of expectation and attainment, is a stimulating suggestion; its further implications remain to be developed.

Sudden shifts. The present theory, it will be recalled, attempts to remedy the defect in Hull's treatment by using his Postulate 4 to describe the acquiring of a goal-expectation, ${}_{s}H_{r_{sG}}$. But if ${}_{s}H_{r_{sG}}$ has to be acquired by Postulate 4, how can we explain the one-trial shift of performance level when reward is increased or diminished? This result depends on the fact that r_{sG} has already been conditioned on the afferent side to the starting box at an intensity corresponding to the original amount of food, S_G. Denoting the starting box by S_S we have: ${}_{s_S}H_{r_{sG}} > 0$. On the critical trial the changed incentive, $S_{G'}$, arouses $r_{sG'}$ of proportional intensity; i.e., ${}_{s_{G'}}H_{r_{sG'}} > 0$. Since S_G and $S_{G'}$ are on a continuum of stimulus magnitude their central counterparts, r_{sG} and $r_{sG'}$, may be said to lie on a continuum of afferent intensity. By the principle of stimulus generalization, therefore, ${}_{s_{G'}}H_{r_{sG'}}$ should give a degree of effective habit strength to the connection ${}_{r_{sG}}\overline{H}_{r_{sG'}}$. Moreover, there is experimental evidence (14) that the gradient of primary stimulus-intensity generalization is quite flat. We may infer that on the next trial after the change of reward a process, ${}_{s_S}H_{r_{sG}}\overline{H}_{r_{sG'}}$, will be activated in the starting box and that it will be of comparable intensity with ${}_{s_S}H_{r_{sG'}}$ aroused in a group trained to $S_{G'}$ from the beginning. From this state of affairs the sudden change in latency and speed follows.

Contrast effect. An adequate theory of contrast effect is much too complex to be attempted here, but a few suggestions may be given. Assumptions are required whereby a change in magnitude of incentive will produce an increment or decrement of motivation beyond that due to a constant amount. We may begin by assuming that the consumption of any quantity of food short of satiation involves some frustration of the unsatisfied need (F_{R_G}); the less food, the more frustration. As the animal's set for this quantity of food is generalized to the starting box of a runway, a corresponding degree of frustration ($F_{r_{sG}}$) will also be aroused there and will act as a negative drive to oppose the positive drive to food.[7] For any given amount of incentive $S_{G'}$ we may compute the

[6] A second anomaly reported by Crespi—the fact that rats reduced from 16 units to no reward ran faster than rats reduced to 1 unit—may be an artifact, since the former group had 14 more runs to 16 units before the incentive was removed.

effective strength of tertiary motivation by substituting values in the following equation:

$$_{S}\overline{H}_{rsG'} = {}_{S}H_{rsG'} - F_{rsG''} \quad [1]$$

Assuming that the downward contrast effect is real, we may deduce it by supposing that F_{RG} is also produced when a portion of R_G is blocked by a sudden reduction in the quantity of food. The greater the reduction, the stronger will be the frustration from this source. It is this added frustration that pulls down the effective motivation of the lowered group *below* that of the group already running to the smaller incentive.

To illustrate, let us suppose that a large quantity of food, $S_{G''}$, is suddenly reduced to $S_{G'}$. Through stimulus generalization, by the mechanism set forth above in deducing sudden shifts, r_{SG} will be aroused in the starting box with an intensity roughly equivalent to $r_{SG'}$, accompanied by $F_{rsG''}$. In addition there will be generated a quantity of F which is some function of the difference between $S_{G''}$ and $S_{G'}$ and which we may designate $F_{rs(G''-G')}$. In this case effective tertiary motivation is determined as follows:

$$_{S}\overline{H}_{rsG'} = {}_{S}H_{rsG''}\overline{H}_{rsG'} - [F_{rsG'} + F_{rs(G''-G')}]. \quad [2]$$

To explain upward contrast, an analogous assumption must be made. If a frustrated animal is suddenly given access to food, some of his F_{RG} will be channeled, or converted, into R_G. Similarly, when S_G is increased the same process will occur. This means that at the start of the next trial a corresponding part of F_{rsG} will be converted into ${}_{S}H_{rsG}$; the greater the proportion of drive converted, the greater the contrast effect. In terms of equation [2] this operation on F_{rsG} is mathematically equivalent to a change of sign. Thus, if S_{G^0} represents a small amount of food suddenly increased to $S_{G'}$, we may write:

$$_{S}\overline{H}_{rsG'} = {}_{S}H_{rsG^0}\overline{H}_{rsG'} - [F_{rsG'} - F_{rs(G'-G_0)}]. \quad [3]$$

The foregoing assumptions are of little value, of course, until they can be put to work; i.e., until the concepts involved are quantitatively related to observables. Such statements in turn depend on more studies like Zeaman's, in which both degree of incentive change and drive level are systematically varied and performance measures plotted against them.

Palatability of Reward. Needless to say, our interpretation of contrast is not intended to be purely ad hoc. To defend it from that charge we should at least be prepared to show that it applies to other dimensions besides the one that gave rise to it. Many years ago Elliott (7) disrupted the maze performance of rats by changing their reward from bran mash to sunflower seed. The effect was sudden and, in comparison with a control group trained to sunflower seed from the beginning, showed a marked "contrast effect" which the author attributed to searching for mash. More recently Young (41) tested the effect of interchanging rewards of different degrees of palatability. Unfortunately his study serves mainly to underline the hazards of gen-

[7] We could arrive at the same result by assuming that F_{RG} is conditioned to the starting box along with food anticipation, thus becoming ${}_{S}F_{rsG}$. Although not indispensable here, this assumption may prove to be a decided advantage in dealing with extinction.

eralizing in this area. Young gave his rats one trial a day to a nibble of food on an open platform and measured their approach time. One group ran to an incentive of high palatability (sugar), another to a lower one (casein). He then shifted half of his animals to the opposite incentive. Under these conditions there was no contrast effect but a difference appeared between the two directions of shift. The upward shift produced an immediate reduction of times, but the downward shift produced no retardation for five days. Whether we call the second phenomenon "wishful thinking" or simply lag, it looks like just the opposite of contrast.

It would be foolhardy to attempt an explanation here. (One ad hoc hypothesis at a time!) But before attributing the discrepancies to a basic difference between amount and palatability of reward we should make sure that they are not due to a difference in the conditions of measurement; e.g., time of food deprivation, type and length of runway, method of scoring, or distance between scaled incentive values. Nor should the possibility of an interaction between amount and palatability be overlooked. Further investigations may show that certain conditions favor contrast and others favor lag, and that these relations are common to all properties of reward.

Introduction of Reward

Evidence on this topic comes from the well-known experiments on latent learning. In a recent excellent review of these experiments Thistlethwaite (36) classified them in four types, of which we are here concerned with the first two. In type I, the Blodgett variety, a group of hungry rats is given a series of trials in a maze with no food in the end-box, followed by a series with food. Evidence for latent learning consists of a sudden drop in errors with the introduction of food in comparison with a control group rewarded with food from the outset. In type II, the Haney variety, the rats are allowed to explore an empty maze for a time before being given regular trials to food. Latent learning is said to have occurred if their initial error and time scores are better than those of a control group allowed to explore a continuous runway.

Positive evidence from both types of experiment has led cognition theorists to the following conclusions, recently upheld by Thistlethwaite after a critical review of the entire field: (1) Habits may be acquired without being "reinforced" by "satisfiers." (2) Habits may be acquired without being reflected in overt performance. To these may be added a third: (3) "Satisfiers" determine the overt performance of habits rather than their acquisition.

All three of these propositions are implicit in the writer's tertiary-motivation theory of reinforcement. In fact, the theory was devised

largely to meet the demands of the latent learning evidence. To evaluate its success in doing so the reader may consult earlier statements (31, 32, 33). Here we shall be concerned with whether the attempt was justified or whether an orthodox reinforcement theory would do as well.

Advocates of what Meehl (26) calls the "Strong Law of Effect" answer the challenge of latent learning in a number of ways:

In answer to proposition (1) above, they point out that the pre-reward period is not devoid of satisfiers. In type I experiments the end-box, as the place of removal from the maze, may acquire secondary reinforcing properties by association with a subsequent anxiety-reducing environment. In both types the maze as a whole may arouse—and satisfy—a rat's need to explore. Whatever the mechanism of exploratory behavior, it seems to be evoked in situations having a common feature, novelty, and to persist until that feature has been eliminated. We cannot therefore deny it status as a need-reducing activity; all we can say in reply is that exploratory satisfactions should be distributed all over the maze instead of being concentrated in certain sections. But it is also possible that a rat may prefer a "through road" to a blind alley, so that even during free wandering he will enter fewer and fewer culs.

In answer to proposition (2) they assert that much so-called latent learning is actually "manifest" (1). The possibilities mentioned above are not sheer speculation. All the type I experiments reported have found some reduction of errors before the introduction of food. Karn and Porter (21) and Reynolds (28) showed how refined observations might detect evidence of learning normally overlooked. Though type II experiments usually provide no measure of such a trend, Haney's (11) data strongly suggest that it existed, since *on the first trial*, before they had ever encountered food in the maze, his experimental rats made less than half as many errors as the controls. One of Buxton's (4) control groups, tested without pre-feeding, confirmed this finding. Finally, MacCorquodale and Meehl (25), using Blodgett's six-unit maze in a direct attack on the question, demonstrated a significant reduction in cul entries from a first to a second exploratory period and to an initial test run.

We must conclude that both "reinforcement" and "improved" performance characterize the pre-reward period. Introduction of reward is actually a change in the quantity and quality of need satisfactions. Yet the reinforcement theorist still has to explain the sharp elimination of errors which sometimes accompanies that change. It is another form of the same problem we faced in connection with changing the amount or palatability of reward.

A third way of trying to solve this problem in Hullian terms is to add more postulates to the system. Thistlethwaite (36) has already disposed of one such suggestion by Meehl by showing that it could not account for Herb's (13) data. I should like to consider three more attempts of this sort that have come to my attention:

1. Thistlethwaite's criticism also applies to the hypothesis recently offered by Meehl and MacCorquodale (27). Their argument starts with the fact of pre-reward improvement, which they interpret as indicating a weak difference in ${}_{S}H_{R}$ in favor of the true path. They assume further that eating food in the maze conditions drive-mediating responses to maze cues. As a result this conditioned drive, multiplied by accumulated ${}_{S}H_{R}$, increases the differences in ${}_{S}E_{R}$ already existing at choice points. The authors tested their hypothesis by substituting for the first rewarded trial in Blodgett's maze A, two minutes of feeding in an extra-maze box. On the next trial the drop in errors, as compared with a control group put in the box without feeding, fell just short of a .05 level of significance.[8]

From my standpoint this interpretation has much to recommend it; indeed, if its authors specified more clearly the nature of the secondary motivation they invoked, their theory might prove to be essentially like mine. In its present form, however, it relies simply on the multiplication of existing habit differentials and thus fails to do justice to Herb's results. That investigator, it will be remembered, modeled her experiment on that of Tolman and Honzik (37), but when she introduced food in the maze she put it at the ends of the blind alleys instead of in the endbox. During the pre-reward trials, while the control group entered more and more blinds the experimental group entered fewer and fewer. Nevertheless, following the introduction of food the experimental group increased its blind-alley entrances significantly more than the control group in any equivalent period.

This abrupt change of slope *in the opposite direction* strongly implies that the previous learning was something other than a differential strengthening of overt responses at successive choice points. It suggests rather that what was learned at any choice point was an antedating reaction to a "dead end" (r_{S_E}) in one direction and to a "through road" (r_{S_C}) in the other. We may assume further that the chief source of differential motivation was r_{S_G} and that in the absence of food this consisted of a return to the home cage or of unimpeded activity. The early reduction of errors may then be accounted for by either or both of two processes: the conditioning of tertiary motivation to r_{S_C} and of frustration to r_{S_E} (e.g., ${}_{S_R}H_{r_{S_C}}H_{r_{S_G}}$ and ${}_{S_L}H_{r_{S_E}}F_{r_{S_G}}$). But the discovery of food in a blind alley should condition a more powerful r_{S_G} to the r_{S_E} aroused at that choice point, thus tipping the balance in favor of entering the blind. By virtue of stimulus generalization the same proc-

[8] Personal communication.

ess should occur to a lesser degree at other choice points and lead to further discoveries of food. Thus we should expect the rapid increase of "errors" which, in fact, took place.

2. Another attempt to buttress Hull's theory with qualifying assumptions was made by Taylor (35). He took as his point of departure my analysis of Blodgett's rectangular-maze data (31). My treatment had a number of faults; for one thing, I failed to take account of the fact that Blodgett's experimental group developed a slight preference for the short path during the pre-food period. Taylor's main objection, however, was on the ground of parsimony, since Hull's system, with the aid of one or more propositions, could be shown to predict Blodgett's results more efficiently than mine. His achievement hinged on the assumption that behavioral oscillation (σ_0) decreases as ${}_sH_R$ increases. But in order to reconcile this assumption with other data Taylor took the position that it holds only under certain conditions. Specifically he suggested that when ${}_sH_R$ undergoes primary reinforcement, σ_0 decreases, but when reinforcement is secondary, σ_0 remains constant. This assumption seems to me untenable for the following reasons:

In the first place Taylor attributed primary reinforcement to the diminution of a primary or unlearned need such as hunger. Yet as examples of decreased variability with practice he used the skilled typist, the expert pianist, and a small group of subjects who were paid for their services in a study of autokinetic movement.

Secondly, Taylor based his argument on the ground that oscillation is one expression of drive and that, therefore, a reduction of primary drive will lead to a reduction of oscillation. This effect becomes conditioned to the drive-reducing situation. "Thus when a hungry animal finds itself in a situation where it has previously discovered food, there is likely to be an immediate reduction in general irritability" (35, p. 84). But this description of the conditions determining a decreased σ_0 could serve equally well as an example of *secondary* reinforcement as that term is commonly defined (18). Primary and secondary reinforcement, as already noted elsewhere (33), are so intimately related that they have so far defied any clear-cut separation. But without an objective criterion for distinguishing between them Taylor's assumption becomes purely ad hoc.

Thirdly, although Taylor himself nowhere defines secondary reinforcement explicitly, he implies that he means by it the diminution of a secondary or learned drive (35, p. 85). But if range of oscillation is a function of strength of drive it should vary with a secondary drive as well as with a primary one. In effect, a drive is a drive whether aroused reflexly or by association. In deriving Blodgett's results Taylor assigned a constant σ_0 to the experimental group before introduction of reward and a decreasing σ_0 thereafter. Since the experimental group in the early trials was admittedly diminishing its exploratory drive it is hard to see any logical ground for the distinction.

The question remains whether my own theory can be amended to

explain the pre-reward as well as post-reward improvement in Blodgett's experimental group. As already suggested, this can be done most simply by recognizing a weak incentive (rs_{G^o})—e.g., escape from maze—associated with the endbox. Let us denote the short path by X and the long path by Y. As a result of differential delay, ${}_{s_X}H_{rs_{G^o}}$ will be greater than ${}_{s_Y}H_{rs_{G^o}}$, thus producing a greater ${}_{s}E_{R}$ in favor of the shorter path. But since rs_{G^o} is weak the difference will be slight, and the overlapping of oscillation distributions will keep the percentage of short-path choices ($\%X$) not far above 50. When food is introduced and a much stronger incentive ($rs_{G'}$) is thus associated directly with the endbox and indirectly with the choice point, the difference ${}_{s_X}E_{R_X} - {}_{s_Y}E_{R_Y}$ is abruptly increased. On the assumption that σ_0 remains unaltered, $\%X$ will also jump.

Of course that assumption may not be valid. My criticisms of Taylor's position were directed at certain inconsistencies; they leave his main suppositions untouched. The conditions determining σ_0 are almost wholly unexplored (18, p. 314). Quite aside from any distinction between primary and secondary motivation, Taylor's idea that σ_0 is a positive function of drive may prove to be correct. In that case the final step of our argument would not follow, since if σ_0 increased, for example, proportionally with ${}_{s}E_{R}$, the X and Y distributions would still overlap to the same degree.

It would still be possible, however, to escape from this dilemma. A way out is suggested by Spence's (34) theory of discrimination learning. Spence held that the excitatory strength of the stimulus complex on either side of a discrimination apparatus is the sum of the excitatory tendencies of the component stimuli. The discrimination is learned when the excitatory difference between the critical cues becomes great enough to outweigh all other differences that might arise in the situation.

In much the same fashion we may assume that for Blodgett's experimental group during the early trials the endbox (S_{G^o}) was only one of an indefinite number of competing incentives, relatively equal in strength, some favoring a right turn and others a left. Due to oscillation, ${}_{s}H_{rs_{G^o}}$ won out in the competition only often enough to produce a small majority of short-path choices. But after food ($S_{G'}$) had been discovered it furnished such a strong incentive that its oscillation hardly overlapped with any of the others; ${}_{s}H_{rs_{G'}}$ therefore almost invariably dominated the animal's choice. Essentially this amendment would suggest that $\%X$ without food is largely a function of the relative potencies of competing incentives; with food in the endbox $\%X$ is largely a function of the difference between the ${}_{s}E_{R}$s associated with the two means of getting there.

3. Finally I should like to discuss Hull's own attempt (17) to tailor his theory to fit latent learning. Buxton (4) had recently published

his disconcerting monograph. Using a type II design combined with a brief pre-feeding in the endbox, he had demonstrated for his experimental groups not only less errors than for the controls but a speed-of-locomotion gradient and a backward order of error elimination *on the very first trial.* Since these phenomena had been deduced from—and accepted as evidence for—Hull's goal gradient hypothesis (15), here was the most formidable challenge to reinforcement principles that had yet appeared.

In 1941 Hull issued a memorandum (17) containing a proof of two propositions embodying two of Buxton's results: the smaller number of errors on the first trial by his experimental rats and the diminishing error gradient as they approached the goal. To the six postulates in his 1937 miniature system he added three assumptions about maze behavior: (1) that animals make perceptual reactions to their environment; (2) that exploratory behavior leads to the reinforcement of habit segments; and (3) that these segments are distributed about the maze by chance. Hull's proof is too intricate to be reproduced here. It follows essentially the same line of argument that he developed (16) in deducing Maier's results on "reasoning," of which he considered Buxton's problem to be a special case. To condense the exposition still further, however, it will be convenient to use my simplified version (32) of the Buxton experiment. That study may be broken down into three stages. In stage 1 the experimental rats were allowed to explore an enclosed single-unit T-maze with choice point *C* and endboxes *A* and *B* to right and left, respectively. In stage 2 they were put directly into one of the two endboxes, say *A*, where they found food for the first time. In stage 3, which followed immediately, they were placed in the starting box and given a free choice; 87.5 per cent chose the path to *A*.

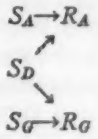
Hull's deduction of this finding, boiled down from his discussions of Maier and Buxton, would run somewhat as follows (in the accompanying diagrams a broken arrow indicates an association in the making; a solid arrow, an association already formed):

1. As a result of maze exploration fractional reactions to the endboxes, r_A and r_B, will be brought forward to the choice point and their proprioceptive impulses, s_A and s_B, conditioned to the respective approach responses, R_R and R_L.

STAGE 1: S_C → $r_B \rightarrow s_B \rightarrow R_L$; S_C → $r_A \rightarrow s_A \rightarrow R_R$

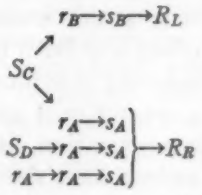
2. Pre-feeding ($S_G - R_G$) at *A* conditions the hunger drive stimulus, S_D, to endbox responses, R_A.

STAGE 2:



3. When the animal reaches the choice point a few seconds later, r_A is still perseverating and is now strongly aroused by S_D. R_R is therefore more intensely activated than R_L.

STAGE 3:



By applying the same reasoning to a succession of choice points in conjunction with his third supplementary assumption, Hull was able to deduce the main features of Buxton's error data. Although his system has advanced far beyond the stage it was in at that time, I assume that his main argument still holds. It rests on a number of characteristic assumptions, of which we may note the following: (a) Learning is a function of satisfiers. (b) Other things being equal, selection between incompatible responses is a function of relative habit strength. (c) Total ${}_sH_R$ is a function of ${}_sH_R$ components. (d) The role of S_D in selection is to add a stronger ${}_sH_R$ component to one response than another.

My own interpretation resembles Hull's up to a point. It includes the conditioning of endbox reactions to choice-point stimuli in stage (1). In stage (2), however, it stresses the association of endbox reactions with eating responses (R_G) rather than with S_D. And in stage (3) it states that the differential arousal of $r s_G$ thus produced at the choice point increases ${}_sE_R$ in favor of the appropriate response. Learning, according to this view, may be largely independent of satisfiers. Selection is determined not so much by relative ${}_sH_R$ as by relative intensity of tertiary motivation, ${}_sH_{rs_G}$. The chief role of S_D is to establish a set toward R_G, which is then differentially blocked or facilitated by external and response-produced—and surrogate-response-produced—stimulation.

But are the two theories actually different in the sense that they lead to different predictions? Since Hull's proof hinges on the greater strength of S_A than S_B, it follows that any means of making it stronger should produce a like result. For example, the starting box could be made more similar to box A. Again, it should be enough merely to put the rat in A without food just before making a choice; the perseverative trace of s_A should then favor a right turn. Evidence against this hypothesis was furnished by another part of my experiment. When rats

were put alternately in both boxes just before testing, half of them fed in the first box and half in the second, 81.3 per cent still chose the food side. Apparently sheer perseveration contributed little if anything to the result. Further negative evidence is the well-known disposition of rats to alternate their responses at a choice point, a tendency that is stronger the shorter the time interval between trials (6, 12).

The second factor in Hull's proof contributing to s_A is the conditioning of S_D to r_A. As long as this is accomplished by reducing S_D itself, R_G is necessarily involved and both theories yield the same prediction. But what if some other need is reduced in the presence of S_D? What if the rats, still hungry, are put in A and permitted to slake their thirst? According to Hull's interpretation, 24 hours later the rats, now hungry but not thirsty, will nevertheless show a preference for A; according to mine they will not.[9] To avoid this deduction Hull would have to supplement his theory with Kendler's "selective principle" (22), according to which a drive stimulus is conditioned only by virtue of its own reduction. A discussion of the present status of this principle would carry us too far afield.

To summarize, we see that Hull has tried to explain latent learning on the assumption that satisfiers determine behavior solely by producing differences in ${}_sH_R$. In my opinion a more satisfactory solution can be found by assuming that a response to a satisfier, rs_G, is itself learned much like any other response; once learned it enters the stimulus complex as one form of motivation to help decide which of two ${}_sE_R$s will find overt expression.

Summary

The present paper has been written to test the writer's tertiary-motivation hypothesis of reinforcement by applying it to an area of research. As the distinguishing feature of this area the independent variable was some property of reward, such as the presence or absence, amount, or palatability of a food incentive. Specifically, results of experiments on the following problems were discussed with respect to their theoretical significance:

1. *Effect of amount and of palatability of reward.* Concerning simple locomotor response, on the basis of Crespi's, Zeaman's, and Young's data, it was concluded that the incentive determines performance level at the limit of improvement but not the rate of approach to that limit. In the case of spatial discrimination, both time and error scores in mastering a choice point have yielded consistently positive results. For visual discrimination results here

[9] The original experiment would, of course, have to be repeated to test the effect of a 24-hour interval between stages 2 and 3.

have been negative. Reynolds' R-hypothesis was criticized, and another attempt was made to reconcile the findings on spatial and visual discrimination with Lawrence's evidence for a "mediating process" taken into consideration.

2. *Effect of changing the amount or palatability of reward on simple locomotor response.* Three behavioral phenomena were noted—sudden shifts, contrast effect, and lag—and interpretations of the first two were suggested. Sudden shifts were explained by means of an application of the principle of stimulus generalization to reward dimensions. To account for contrast effect an intervening variable of frustration was introduced; its quantity was assumed to vary as a function of both amount of reward and degree of change in that respect.

3. *Effect of introducing reward on maze performance.* Attempts to interpret latent learning data in terms of reinforcement theory were evaluated, especially those of Meehl and MacCorquodale, Taylor, and Hull. Implications of the writer's hypothesis were suggested as an alternative in each case.

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COMMENTS ON RAZRAN'S DISCUSSION OF STIMULUS GENERALIZATION

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Razran recently has made a valuable contribution by his summary of the literature on stimulus generalization (5). However, there are several points in Razran's interpretative discussion with which the writer is in disagreement and which he feels are in need of further clarification.

1. In the first place, Razran appears to be unduly concerned with the effect of variability upon empirically obtained generalization gradients. For example, with respect to Hovland's study, he says, "*Thus, not a single [one] of Hovland's 20 subjects revealed a consistent gradient of CR generalization and only one of the three gradient-determining differences was statistically reliable.*" The writer does not mean to minimize the importance of variability in behavior theory. However, the fact that variability may be superimposed upon underlying lawful relationships is in no way an argument against the existence of such relationships. This fact of behavioral variability has been well recognized by Hull with the use of his concept of behavioral oscillation ($_sO_r$) (3). Within the framework of Hull's theory, this concept would, in fact, lead to the prediction that for individual subjects, many inversions would be obtained in the determination of generalization gradients. The number of inversions which would be expected for any particular stimulus dimension would depend upon the range of the oscillation function, the form of the generalization gradient for that dimension, and the particular points on that dimension which were selected for test. The flatter the generalization function, the greater would be the expected frequency of inversions. It would also be predicted that there would be fewer inversions between stimuli which are far apart on the dimension than there would be between stimuli which are close together on the dimension. In general, this prediction seems to be verified by Razran's findings. The occurrence of inversions for individual subjects in generalization studies is almost directly analogous to the result that would be obtained from the psychophysical method of constant stimuli if only one or two judgments were obtained for each stimulus or stimulus pair. While a normal ogive may satisfactorily be fitted to data for the constant method if the number of judgments is large, a great many inversions are obtained if the number of judgments is small. Thus, while variability may obscure the lawful relationship which exists, it certainly is not used as an argument against its existence.

In addition to variability, another complicating factor enters in at this point to determine the nature of empirical generalization functions

for individual subjects. This is the order in which the test stimuli are presented. This is an inherent difficulty in any generalization study in which the same subjects are tested for generalization at different points on the stimulus dimension. In another paper, the writer (1) has pointed out that, strictly speaking, only one valid measurement of generalization may be made for a particular subject. This is because of the possible effects of one test trial on subsequent ones. If test trials are reinforced, such reinforcement would have the effect of extending the generalization gradient. On the other hand, if test trials are not reinforced, the picture is complicated by the effects of differential reinforcement. Hovland's (2) solution to this problem was to counterbalance the order of presentation of the test stimuli. Thus, the generalization curve for any particular subject would be determined jointly by the generalization function and by the order in which the particular stimulus was presented. With this procedure, the fundamental law of generalization for this dimension could be determined only by determining the average response to each stimulus with order of presentation counterbalanced.

Similar reasoning with respect to variability should be applied to the argument concerning the lack of statistical significance between adjacent points when gradients are based on the results of groups of subjects. The obtaining of statistically significant differences depends upon the amount of variability, the size of the sample upon which the means are based, and the magnitude of the difference between the means. The magnitude of the difference between the means, in turn, depends upon the form of the generalization curve and the distance between the two stimuli on the stimulus dimension. Since the variability is usually relatively great and the size of the samples relatively small, it is not surprising that differences between adjacent or near points on the continuum are not statistically significant. The fact that statistically significant differences are found between points more widely separated on the continuum would appear to be proof that such gradients do, in fact, exist. The writer suggests that significance of trend tests based upon analysis of variance, such as the tests described by Linquist (4), would be a more appropriate type of statistical analysis to apply in this situation.

2. The second point to be discussed here is Razran's contention that generalization gradients are not generated at the time of original conditioning but that "all effects of generalization are generated during tests of generalization." Since the existence of generalization gradients can be known only by performing the operations necessary for their measurement, it would appear that it makes little difference whether the theorist assumes that the generalization gradient is generated at the time of original conditioning, at the time of the test, or at some point in between. Experiments involving additional training or changed

instructions subsequent to the original conditioning can have no bearing upon this question since such procedures might well be expected to modify the nature of the response in either case. Since generalization gradients may be determined only by actual test, statements to the effect that they are developed at the time of conditioning or at the time of testing appear to be operationally meaningless—at least insofar as behavior theory is concerned.

3. The final point to be considered is with respect to Razran's experiments on semantic generalization. While these experiments are of great importance and interest, they are not cases of primary stimulus generalization as that term is used by such writers as Hull and Spence. Semantic generalization may more properly be regarded as a form of secondary generalization or "mediated" generalization. It is quite likely that the mechanism underlying semantic generalization is quite different from the mechanism underlying primary stimulus generalization. While primary stimulus generalization gradients are assumed to depend upon physical similarity of the stimuli, it is quite likely that semantic generalization depends upon learning by the subject in which the conditioned stimuli and generalized stimuli have become associated. The postulation of some type of mediating process or response may be necessary to account for this phenomenon. While in no way minimizing the importance of mediated generalization, the writer would contend that a consideration of such phenomena is not necessarily relevant to the problem of primary stimulus generalization.

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REPLY TO GRICE'S "COMMENTS ON RAZRAN'S DISCUSSION OF STIMULUS GENERALIZATION"[1]

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At the time of the writing of my article I was familiar with Hull's *Principles of Behavior* and the postulate of behavioral oscillation, as well as with the text by Hilgard and Marquis and their concept of learned mediation as applied to the generalization from a stimulus object to its denoting word in an experiment by Smolenskaya (4, p. 201). I am grateful to Dr. Grice for the reminder but do not know in what way his comments call for any modification of my article and views. I shall take up Grice's three "points" in an order reversed from the one in which they are presented and will add two "points" of my own:

1. The statement that my experiments on semantic generalization are not unqualifiedly comparable with those of sensory (and spatial) generalization has my sympathy. But at no time did I say differently, as indeed I even questioned (after Lashley) the unqualified comparability of human sensory with animal sensory generalizations (6, p. 256; 7, p. 353). As the reader will remember, my chief source of evidence against the generalization views of Pavlov and Hull were results—numerous results—of Pavlov's own animal studies, and it is only when striking similarities between animal and human being and between sensory and semantic generalization had been unravelled that *some* common mechanisms were suggested. The obvious dependence of semantic generalization upon previous learning and association—to which Grice devotes half a dozen obvious sentences—hardly needed indication in an article whose task was to determine whether *all* generalization is not due to previous (pre-experimental) learning and association.

2. The argument (itself faulty; see 6, 8) of testable differences between generalization occurring during original conditioning and during subsequent testing is immaterial to my main thesis of suggesting a hypothesis that would, besides obviating a practically untenable neurophysiology, substitute present and on-the-spot and overt interactions and contiguities for past and remote and latent perseverations and potentialities. Should not a behavior theory, other things being equal, prefer that? And what about the "operational" advantages of having one less postulate to worry about?

3. The discussion (again quite obvious) of behavioral oscillation

[1] G. R. GRICE. Comments on Razran's discussion of stimulus generalization. *Psychol. Bull.*, 1951, 48, 150–152.

and of order of test stimuli presentation is equally of little relevance, since I do not deny lawfulness in CR generalization but merely claim the existence of a few crude categories rather than that of a continuum of j.n.d.'s or irradiations. And this is unopposed by what Grice says and, I repeat, will generally be admitted by unbiased readers of the total evidence, even though some readers might want in *some* instances more statistics to mediate between categories and continua, and others might put little—or insufficient or no—credence in my subjects' introspections. (I sometimes think that experimenters' ratiocinations do more harm—or less good—than subjects' introspections; after all, Descartes lived before Wundt.) Perhaps the word "bias" may serve as a clue to the differences between the thinking of Grice and myself. Grice, like a number of other indoctrinated behavior theorists, seems to attach a privileged status to Hull's postulates. To them these postulates are presumably true unless there is clear-cut evidence against them, while to me such postulates should not stay on the books unless there is clear-cut evidence for them (see Skinner for ambiguities of the term "postulate" in this connection, **9**). Nor do I always favor the dyadic dialectics of these postulates, even though it has been customary since the days of Zoroaster to make Ahriman, the god of darkness, shade the lines of Ohrmuzd, the god of light.

4. I recently analyzed a dozen more theses from Pavlov's laboratory as well as some more of my own data, and both suggest a supplementary hypothesis to the one of categorizing. It may well be that CR transfers that are now subsumed under primary stimulus generalization and under mediated generalization are really cases of residuals of multiple co-conditioning, as implied in Pavlov's studies of compound conditioning and as specifically stated by Hull (after Thorndike) as "generalization by means of identical stimulus components" (**5**, pp. 190–191). If a dog is conditioned to a metronome and a light and a tactile vibrator, and then shows CR transfer to a light and a tactile vibrator or to a light and a whistle and a tactile vibrator, there is little dispute about the mechanism of transfer. But it is obviously equally true that a dog conditioned to a certain tone frequency is also co-conditioned to the intensity, the duration, and the locus of the tone—besides of course other aspects in the CR situations—and when the tone frequency is changed, a good number of results of Pavlov's laboratory indicate that the CR transfer here is much like that obtained in the preceding case of compound conditioning. Again, in human subjects—as well as in some animal situations—the situation may be further modified through what Brogden called sensory pre-conditioning (**2**, **3**). Thus, if my subjects have been conditioned to the sound of a metronome (without seeing the metronome) or to the sight of the word "metronome," and then manifest CR transfer to the sight of the metronome (without sounding it), there is reason to believe—and subjects' introspections support the

belief—that the transfer is due to the fact that in the original CR situation the sight of the metronome was present and co-conditioned as a stimulus component through sensory pre-conditioning. Or, generalizing and reducing to lowest terms, we may say that in one case the conditioning is to "AB" and the generalization testing is to "AX" and that in the other case the conditioning is to "(A)B" and the generalization testing is also to "AX"—where "A," "B," and "X" are stimulus components (or aspects or even configurations) present during original conditioning or during subsequent testing, and "(A)" is a stimulus component invoked during original conditioning through co-conditioning—and thus obviate both primary stimulus generalization and mediated generalization.

5. One other point. Gradients of generalization in the manner of Pavlov and Hull contain, among other things, some unusual and contradictory assumptions about the capacity of organisms. Such gradients of generalization are obviously at the same time gradients of differentiation and thus assert that organisms are capable of developing in a few trials multitudes of discriminative reactions. Yet it usually requires a considerable amount of training to form even one discriminative reaction in the same organisms by the method of contrasts. And no human subject, I am sure, could emulate, with the best CR technique and voluntary effort, the reported gradient performance of any of Anrep's three dogs (1). I defy any psychologist to do so.

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A STATISTICAL CONSIDERATION IN PSYCHOLOGICAL RESEARCH

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In much psychological investigation, the experimenter is confronted with several variables. He hopes that certain significant relationships, or differences, may exist between various pairs of these variables without having logical grounds, a priori, for stating just which pairs they might be. To test his idea, the investigator may calculate all possible correlation coefficients, critical ratios, *t*-ratios, *F*-ratios, or chi-squares—depending upon the experimental design. Those pairs of variables which show a significant relationship, or difference, are then investigated further—or the experiment may stop at this point with conclusions based upon the "significant" findings.

While the statistically rigorous investigator may find some quarrel with the above procedure (**1**), frequently it is almost the only avenue of investigation open. This is particularly so where the subjects are not rats or sophomore psychology students but are instead adults in the clinic or industry.

Assuming, then, that the above procedure has been followed, there results a mass of data which must be analyzed. In this analysis, certain principles regarding significance of findings must be observed (**3**). In the simple experiment where one correlation, *F*-ratio, *t*-ratio, or chi-square is calculated, the experimenter selects the 5 per cent, 1 per cent, 0.1 per cent, or some such level of significance and decides that his results are or are not significant. In the "blind analysis" type of experiment the procedure is not so simple. Using correlations as an example, were 100 *r*'s to be computed, about five of them would differ significantly from zero at the 5 per cent significance level even though there was actually no correlation between any of the pairs of variables—this by definition of the 5 per cent significance level. Thus, the experimenter who finds two *r*'s, out of the 14 he has calculated, significant at the 5 per cent level may not have two events each one of which would happen only five times in 100 trials. Instead, he probably has one event which would happen about 15 times in 100 trials on the basis of chance alone. This probability was based on the binomial expansion (**2**).

Whenever a large group of statistics is collected in the "blind analysis" type of experiment, the number of significant statistics should be tested by the binomial expansion to determine if that number is suffi-

ciently great to indicate a non-chance occurrence. (This applies to groups of correlation coefficients, *t*-ratios, critical ratios, *F*-ratios, and chi-squares.)

Of course, expanding the binomial to determine the probability involved can be a laborious task. Although certain tables are available

TABLE I

PROBABILITY OF OBTAINING *n* OR MORE SIGNIFICANT STATISTICS BY CHANCE IN A GROUP OF *N*

(Where statistics are significant at 1 per cent level.)

n	1	2	3	4
N				
1	.0100			
2	.0199	.0001		
3	.0297	.0003		
4	.0394	.0006		
5	.0490	.0010		
6	.0585	.0015		
7	.0679	.0020		
8	.0773	.0027	.0001	
9	.0865	.0034	.0001	
10	.0956	.0043	.0001	
11	.1046	.0052	.0002	
12	.1136	.0062	.0002	
13	.1225	.0072	.0003	
14	.1313	.0084	.0003	
15	.1399	.0096	.0004	
16	.1485	.0109	.0005	
17	.1571	.0123	.0006	
18	.1655	.0138	.0007	
19	.1738	.0153	.0009	
20	.1821	.0169	.0010	
21	.1903	.0185	.0012	.0001
22	.1984	.0189	.0013	.0001
23	.2064	.0220	.0015	.0001
24	.2143	.0239	.0017	.0001
25	.2222	.0258	.0019	.0001

which expand $(p+q)^n$ where $p=.05$ or .01, it is the purpose of this paper to present tables which will furnish the psychologist with the probabilities of obtaining *n* or more significant statistics in a set of *N* on the basis of chance alone. It is to be understood that these tables should not necessarily be referred to where a priori, rather than a posteriori, logic

TABLE II

PROBABILITY OF OBTAINING *n* OR MORE SIGNIFICANT STATISTICS BY CHANCE IN A GROUP OF *N*

(Where statistics are significant at 5 per cent level.)

n	1	2	3	4	5	6	7
N							
1	.0500						
2	.0975	.0025					
3	.1426	.0072	.0001				
4	.1855	.0140	.0005				
5	.2262	.0226	.0012				
6	.2649	.0328	.0022	.0001			
7	.3017	.0444	.0038	.0002			
8	.3366	.0573	.0058	.0004			
9	.3698	.0712	.0084	.0006			
10	.4013	.0861	.0115	.0010	.0001		
11	.4312	.1019	.0152	.0016	.0001		
12	.4596	.1184	.0196	.0022	.0002		
13	.4867	.1354	.0245	.0031	.0003		
14	.5123	.1530	.0300	.0042	.0004		
15	.5367	.1709	.0362	.0055	.0006	.0001	
16	.5599	.1892	.0429	.0070	.0009	.0001	
17	.5819	.2078	.0502	.0088	.0012	.0001	
18	.6028	.2265	.0581	.0109	.0015	.0002	
19	.6226	.2453	.0665	.0132	.0020	.0002	
20	.6415	.2642	.0755	.0159	.0026	.0003	
21	.6594	.2830	.0849	.0189	.0032	.0004	
22	.6765	.3018	.0948	.0222	.0040	.0006	.0001
23	.6926	.3206	.1052	.0258	.0049	.0008	.0001
24	.7080	.3392	.1159	.0298	.0060	.0010	.0001
25	.7226	.3576	.1271	.0341	.0072	.0012	.0002

or the experimental design points to several pairs of variables which should be significantly similar, or different.

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STONE, C. P. (Ed.) *Annual review of psychology.* Stanford, Calif.: Annual Reviews, Inc., 1950. Pp. vii+330. $6.00.

The recent increase in the quantity of psychological publications, as well as the fission of psychology into many diverse areas, has prevented most psychologists from maintaining an adequate panoramic view of the development of their science. Because of this condition, there is urgent need for some sort of periodic review of contemporary psychological literature which will enable specialists in one branch of the subject to keep in touch with other areas. The first volume of the *Annual Review of Psychology* suggests this need will be competently met.

This volume contains reviews of recent psychological literature (1948–1949) in 18 fields of psychology. The aims set forth by the Editorial Committee were twofold: (1) that the reviewers "cover principally publications . . . which [they] considered most noteworthy. . . .," and (2) that the reviewers "adopt an interpretative and evaluative approach to the literature they selected for review." What is meant by "noteworthy" obviously depends upon individual judgment as to what is the most appropriate research strategy to pursue at the present stage of development in the science of psychology. It therefore seems justifiable to demand of each reviewer an explicit statement of his rationale for the inclusion and exclusion of publications. Many of the authors supply such a statement, but most do not.

Those readers who are familiar with the Robinson-McGeoch-Melton tradition in the psychology of learning will be surprised to discover that Arthur W. Melton, in his top-notch chapter on recent learning publications, chooses to organize it "in terms of the major theoretical problems of the learning process, including transfer of learning and retention, but with emphasis on the theoretical differences of the Tolman and Hull groups." This writer would be the last to question the wisdom of the selection of such an organizing principle, but it is quite possible at our present primitive stage of development that important findings need not be instigated by *theoretical differences.* This comment in no way detracts from the general excellence of Melton's chapter.

The method by which the various writers chose to deal with the literature under review varied over a wide range. Robert L. Thorndike, in one of the best chapters, "Individual Differences," does much more than merely report the findings of the numerous studies. He comes to grips with the ideas behind the studies and does not hesitate to be critical when necessary. Robert R. Sears, "Personality," and Carroll L. Shartle, "Industrial Psychology," in two other good chapters, follow somewhat the same procedure. Because Sears reviews only 39 publica-

tions, he is able to deal more intensively with the literature he cites. One wonders, however, why the field of personality, which has captured the interest of such a large number of psychologists, has the shortest list of references.

The field of discriminal processes is handled expertly by Neil R. Bartlett, "Vision," Edwin B. Newman, "Hearing," and Frank A. Geldard, "Somesthesis and the Chemical Senses." It is interesting to note Bartlett's concern about the neglect of some important visual problems due to the tendency of Federal agencies to support research most likely to have practical application. Newman's chapter attempts, unlike most other reviews, to integrate apparently discordant data. His scholarship is impressive, as is his pleasant prose; the latter, unfortunately, appears in this volume much too infrequently.

All of the reviews are adequate and informative. Some are particularly interesting because they reflect so well the existing state of the field which is reviewed. Jerome S. Bruner's chapter mirrors the disorganization of present-day social psychology. It would have been helpful if Bruner had attempted to relate more explicitly the research programs of social psychologists to their underlying assumptions as to the nature and aim of science. Perhaps then a somewhat clearer picture of contemporary social psychology would have emerged.

The chapters in the clinical areas would benefit by the elimination of overlapping. There are two chapters on therapy ("Clinical Methods: Psychotherapy" and "Counseling Methods: Therapy"), as well as two chapters on diagnostics ("Clinical Methods: Psychodiagnostics" and "Counseling Methods: Diagnostics"). It would be wise for future volumes to compress these four chapters into two, one on psychotherapy, the other on psychodiagnostics. Howard F. Hunt's chapter on psychodiagnostics and William V. Snyder's review of psychotherapy are both excellent.

David A. Grant's treatment of "Statistical Theory and Research Design" is very complete but, unfortunately, pitched at a rather high level. Most psychologists must keep up with new statistical techniques, and it would have been quite helpful if the review of statistical procedures had somehow been designed to meet their needs.

The editors were no doubt pressed to meet a deadline. Several errors in the lists of references testify to this. It would be helpful if the future volumes in this series presented the bibliographies in alphabetical order. Hunt's chapter, in the present volume, is the only one following this procedure.

One cannot read this book in its entirety without reflecting upon the expansiveness of contemporary psychology and its concomitant lack of cohesiveness. To some extent the book intensifies the confused feeling common to so many psychologists attending APA meetings who seek a rationale for the existence of one organization representing all

of psychology. Although not convinced that his opinion as to the mutual interdependence of the different levels of psychology is justified—either on methodological or on practical grounds—this reviewer nevertheless feels that it would be most unfortunate if the definite trends toward fission in psychology were to continue at their present pace. Do not psychologists in all areas have common problems? Can we not assist each other in the solution of some of these problems? The frequent "ethnocentric" complaints heard from all the "sub-cultures" of our psychological community stem mainly from lack of information. If all psychologists read the *Annual Review of Psychology* in its entirety—as this writer believes they should—then better understanding might develop between workers in different fields of psychology. Perhaps here will lie the major contribution of this volume.

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SARGENT, S. S. *Social psychology: an integrative interpretation.* New York: Ronald Press Co., 1950. Pp. x+519. $4.50.

Sargent's book adds another volume to the spate of competent social psychology texts that have appeared since the war. Eclectic and relatively elementary, it will recommend itself for its broad coverage, up-to-dateness, and general good sense rather than for theoretical incisiveness or detailed treatment of important research areas. Greater emphasis is given to sociological contributions than is usual in a psychologist's social psychology; indeed, most of the present and traditional preoccupations of social psychologists of all persuasions come in for some attention. The result has both the virtues and the limitations of an informed eclecticism.

Part I treats the socialization of the individual, taking as a starting point the heredity-environment issue in social psychology and proceeding, via a consideration of anthropological and sociological evidence concerning social influences on personality, to an elementary discussion of the process and products of social learning. Part II is devoted to the various theories of social motivation, with primary attention to psychoanalytic mechanisms and to Allport-Cantril-Sherif ego theory. In Part III, the "patterning of social behavior" is considered in regard to language and communication, the forms of social interaction, social groups, social roles, and leadership. The section ends with the presentation of a simple "field theoretical" orientation in which social behavior is attributed to three broad categories of interrelated determinants: personality, the social situation, and the individual's perception of the situation. Applications of social psychology to the understanding of social phenomena comprise Part IV, which completes the book. The chapter headings indicate the miscellany of topics singled out here for treatment: public opinion, propaganda, mass behavior, social change

and social movements, group differences and prejudice, and applications in the area of social issues. A final summary chapter attempts a judicious appraisal of pure and applied trends in current social psychology.

The chapter on social roles adds most toward clarifying present terminology and assumptions. Yet Sargent's proposed definition seems not to avoid entirely the existing confusions that he depicts. Since the patterned social expectations concerning a person's behavior, the situational patterning in his behavior itself, and situationally organized facets of his self-image may vary independently, it is hardly satisfactory to embrace them as he does under a single definition. Also noteworthy are the particularly clear-headed analysis of hereditary and environmental influences, and the brief but comprehensive survey of action research associated with the Society for the Psychological Study of Social Issues.

Certain gaps stand out in the generally full coverage. The extensive recent research on the social psychology of the cognitive processes is merely mentioned in passing. The California studies of personality dynamics in prejudice are ignored, except for a notation on supplementary reading. Research methods, discussed briefly at the outset, are given rather little attention thereafter, and the interpretative conclusions of studies are typically cited without much detail as to the data on which they were based. Supplementary material is clearly called for if this text is to be employed in courses with a research emphasis.

A degree of superficiality is the necessary cost of attempting so broad a scope. More serious as a limitation is the loose integration characteristic of Sargent's eclectic approach. Conceptually, the lion is sometimes made to lie down with the lamb, disregarding possible incompatibilities, as when Miller and Dollard terminology is coupled with what is essentially a cognitive field theory. There is rather little theoretical interpenetration among the major sections of the book. Roles, attitudes, and socialization are discussed in different contexts, and the treatment of applications is little tied to the earlier theoretical sections. Conventional categories like imitation and suggestion, whose usefulness has recently been challenged, are preserved intact, and the book is replete with current classificatory schemes of somewhat dubious usefulness.

References to the literature, given in footnotes, are extensive and up to date. A helpful teaching aid is provided in references at the end of each chapter to the relevant sections of Newcomb and Hartley's *Readings in Social Psychology* and Britt's *Selected Readings in Social Psychology*, as well as to more intensive sources. The presentation is clear and well organized, and the frequent recapitulations and summaries should prove particularly helpful to the student.

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Vassar College.

GURVITCH, GEORGES. (Ed.) *Sociometry in France and the United States. A symposium.* New York: Beacon House, 1950. Pp. vii+261. $7.50.

The first three numbers of Volume 12 of the journal *Sociometry* have been bound in book form to make an aniversary volume. Roughly the sixteen articles constituting the book can be divided into three types: seven contributions of a speculative, theoretical or critical nature dealing with sociometry, microsociology and axiology; three articles giving the history, application, and present centers of sociometric work; and six reports of research in which sociometric techniques have been employed.

Much of the theoretical and speculative discussion is uninhibited in its praise of the sociometric techniques. J. L. Moreno unabashedly takes credit for saving all the social sciences from sterility and also for establishing the basis for a dynamic social revolution. "With the cooperation of 'all' the people we should be able to create a social order worthy of the highest aspirations of all times. This, and this alone is the meaning of revolutionary, dynamic sociometry" (p. 254). His critical analysis of Marxism and his "sociometric theses" of social change are of real dialectic interest; but their experimental and practical soundness are, in the reviewer's opinion, yet to be demonstrated. Nevertheless, Moreno does make a cogent analysis of some of the defects of much modern sociological and social psychological research.

Georges Gurvitch, in an article entitled "Microsociology and Sociometry," presents a detailed description of his concept of microsociology. In general, Gurvitch is favorably inclined toward the sociometric approach but laments the extent to which the "We-ness" of social relations is untapped by sociometry. René Zazzo considers the relations between sociometry and psychology. While recognizing the value and uniqueness of Moreno's work, Zazzo believes that it overemphasizes the affective nature of interpersonal relations. Leopold von Wiese discusses sociometry as represented in Moreno's *Who Shall Survive?* Paul E. Johnson presents a completely speculative neotheology. It remains for Pitirim Sorokin to examine critically Moreno's concepts of spontaneity and creativity. He raises a number of critical questions such as the extent to which creativity and spontaneity, as measured by Moreno's techniques, are reflctions of *genuine* spontaneity and creativity, and not simply manifestations of an uninhibited superficiality. In a final article Moreno attempts to answer these criticisms of sociometry.

Helen Jennings gives an historical account of the military use of sociometric and situational tests in a number of countries. She praises the Office of Strategic Services program for retaining a large element of spontaneity in its assessments. A listing of sociometric research and work centers in France is given; and Zerka Toeman presents a "History of the Sociometric Movement in Headlines."

The research reports are of great interest both because they tend to give an operational definition of one aspect of sociometric work and because of the results they present. Paul Maucorps reports on an intensive sociometric investigation of thirty-five officers being trained in a French military school for staff work. Bengt Danielsson's study of an Amazon Indian group suggests an operational definition of "key persons" in simple groups and also presents some positive evidence of the relationship between verbal and nonverbal behavior. Charles Loomis and Reed Powell report a study of the different types of class status in a Costa Rican peasant proprietor area and in a hacienda community. The lack of communication between social classes in the centralized rural plantation, in contrast to the mobility of the proprietor group, is noteworthy. George Lundberg and his co-workers present the results of a study of the attraction and repulsion patterns in a university. Apparently propinquity is the major determining factor in these patterns. Henry Maas reports a not altogether successful attempt to relate changes in social attitude to changes in sociometric choice. Finally the excellent study of buddy ratings by Robert Wherry and Douglas Fryer is reprinted from *Personnel Psychology*. The authors were able to gather a number of criteria of leadership at the Signal Corps Officer Candidate School and, by careful analysis, show that anonymous nominations constitute their purest measure of leadership.

For those interested in social psychology and who do not have the journal *Sociometry* available, this book offers a stimulating review of this growing field. The reviewer cannot help but add that the book contains many typographical errors and parts of some charts are illegible; the price, $7.50, seems excessive.

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ROBACK, A. A. *Personality in theory and practice.*[1] Cambridge, Mass.: Sci-Art Publishers, 1950. Pp. 437. $3.75.

This is another original and thought-provoking work by the learned author of the monumental treatise, *The Psychology of Character*. In his characteristic style he leads us with an almost deceptive facility through the maze of the scholarly and scientific contributions to the multi-chambered castle of personality and then offers his own definition: "Personality is the integrated organization of all the cognitive, affective, conative, and physical characteristics of an individual as it manifests itself in focal distinctness *and carrying a special meaning* to others" (the italicized words refer to the addition to his original

[1] The original book, now out of print, *Personality, the crux of social intercourse* (1931), constitutes about a fifth of the present text. A review of it by G. W. Allport appeared in *Psychol. Bull.*, 1932, 26, 359–360.

definition in Warren's *Dictionary of Psychology*). This latter phrase has been added in accordance with his view that personality, like character, should be evaluated. In this respect, then, Roback's view is much like May's concept of "social-stimulus value," and likewise has the weaknesses which have been noted by several writers, notably, G. W. Allport. One might also criticize the word "integrated" in Roback's definition, connoting as it does *un fait accompli*, by asking to what *degree* integration must be achieved in order that the term "personality" may be applicable. Are children, for example, automatically disfranchised as possessors of personality because their "integrations" are unfinished?

After a discussion of "trait" as the unit of personality which follows along the lines drawn by G. W. Allport, we are treated to a challenging chapter on the determinants of personality. Here Roback seeks to lay bare the implicit assumptions of the strictly environmentalistic interpretations. But he is not so naïve as to ignore completely the effects of environment. Thus, he writes, "our dispute is not with a moderate geneticism, but with an immoderate environmentalism." Undoubtedly psychologists—perhaps, more especially American—under the combined impact of behaviorism, sociology, and cultural anthropology, have been neglecting, or worse still, ignoring the nativistic determinants of personality. But, ultimately, we must make a choice between these two contenders. Shall the choice be on the basis of some aesthetic or metaphysical quality inherent in the one or the other, or on their respective merits to initiate crucial experiments or empirical studies to prove or disprove themselves? The latter alternative, of course, is more in keeping with our scientific interest and should become a competitive motive between the two camps. Roback's uncompromising but witty polemic sets well the stage for the first round in the comeback of the undying instinctivist position.[1]

Perhaps the most original ideas in the book are to be found in the chapters on "Meaning and the Personal Idiom," "The Adequacy Valence of Personality," and "The Criterion of Personality." In the first of these Roback insists—not unlike the Freudians—that our seemingly meaningless acts which, at first blush, appear as *disjecta membra*, have, nevertheless, a meaning peculiar to ourselves—our "personal idiom." Furthermore, were it not for the differences in these acts from person to person we could scarcely be aware of the "aroma" of personality. Good and well. But how shall we ascribe meaning, and from whose frame of reference? Here, however, we are left to our own devices. In the following chapter Roback defines his concept, "adequacy valence," as "a personality nucleus which . . . represents an ideal relationship

[1] Another recent provoking treatise written from the same vantage point is Helge Lundholm's *God's failure or man's folly?*, Cambridge, Mass.: Sci.-Art Publishers, 1950.

between the individual and society." Thus defined, he then seeks out "the ingredients of our personality which together constitute human worth. . . . " He finds it in a triad: independence, insight, and sentiment. This selection, of course, is arbitrary and suffers accordingly the fate of all such selections in ethics as elsewhere. Nevertheless, the idea underlying Roback's selection is commendable. Psychologists measure a person's intelligence, attitudes, aptitudes—why not his total worth? In the chapter, "The Criterion of Personality," the author denies that personality evaluated is coterminous with character. Rather, the latter "connotes a fighting spirit on behalf of the regulative principles," while the former connotes sympathy and "the extent of the influence a personality exercises for the general good over a period of time." It is hard to see the boundaries that Roback tries to erect here, for the "regulative principles" (liberty, justice, truth, world progress, etc.) seem to mean the same thing as "the general good." Nevertheless, people do evaluate one another as "good," "bad," etc., and psychologists, armed with refined techniques, might well profit by a concern with these worthy matters. Whether they call it ultimately evaluation of character or evaluation of personality should not matter. What matters is evaluation!

In fine, this book is like a gadfly dissatisfied with much of present day psychology. Its needling questions ought to go far towards disturbing psychologists out of their complacency about "accepted" answers and prodding them into a rethinking of their research ends and research means. This book probably will not be acceptable as a college text to most psychologists of personality, but it might well prove to be of even greater usefulness if they themselves became its students. Indeed, no one interested in psychology should miss reading it.

PERCY BLACK.

Institute of Child Welfare, University of Minnesota.

MAY, ROLLO. *The meaning of anxiety.* New York: Ronald Press Co., 1950. Pp. xvi+376. $4.50.

This is a book which paints the problem of anxiety on a wide canvas. The author sees anxiety as "a pervasive and profound phenomenon" of our time, a "nameless and formless uneasiness that has dogged the footsteps of modern man." Today it has become a conscious and explicit problem, recognized as central and urgent not only by students of neurosis but also by poets and philosophers, historians and social scientists, teachers and ministers, in fact by all who seriously concern themselves with human affairs. Heightened by the grim external circumstances of the immediate present, anxiety is deeply rooted in those rapid cultural changes which produce "inner confusion, psychological disorientation, and uncertainty with respect to values and acceptable standards." We live in an age of anxiety and we need to be as clear as

we can about it. To this end the author brings together the theories of anxiety "offered by modern explorers in different areas of our culture," searches them for common elements, and attempts to construct a coherent and comprehensive synthesis. Though written by a psychologist, the book is not intended only for professional colleagues but for all serious students of human problems.

The generous scope is apparent throughout the first half of the book. From the philosophical predecessors of modern theorists of anxiety the author chooses Spinoza, Pascal, and especially the subtle Kierkegaard. His biological sources include Goldstein, Cannon, and the current workers in psychosomatic medicine. Animal and child psychology are searched, then psychotherapy, an order of presentation which gives one of the odd sensation of jumping from Mowrer back to Freud but which valuably widens the now well worn path from Freud through Adler to Horney and Sullivan. The cultural approach is chiefly represented by Tawney, Fromm, and Kardiner, but wide historical reading also informs this stimulating chapter. There is a refreshing freedom from departmentalized thinking; rather, the progress through this series of skilfully condensed surveys has the character of a continuous journey which places the problem of anxiety in the full perspective of human life.

The first and larger section of the book is brought to a close with a chapter entitled "Summary and Synthesis of Theories of Anxiety." It is more summary than synthesis. My first feeling, however, that the author had failed to achieve a tight and logical synthesis was soon tempered by the realization that he had perhaps hoped for more synthesis than the nature of the problem allowed. Part of the problem of anxiety is to discover the nature of the reaction itself and to work out both the gross and subtle effects on personality produced by the necessity for defense. Another large part, it seems to me, consists of pointing out the myriad circumstances in which anxiety is aroused—myriad because personality is essentially complicated, ramified, set in a host of physical and cultural circumstances. Freud, Horney, Kierkegaard, Fromm point out somewhat different circumstances under which threat arises; all are "true" and they do not have to be synthesized. My only serious criticism of this chapter, therefore, is of the author's distinction between anxiety and fear, a point to which he devotes considerable discussion. He makes the distinction by means of two criteria: (1) anxiety is diffuse and objectless, fear specific and object-related; (2) anxiety is provoked by threat to some value felt essential to existence as a personality, fear is provoked by threat to some merely peripheral value. I do not believe that these two criteria can be worked simultaneously, and in any event I wonder if the distinction between anxiety and fear is not more verbal than biological.

The second part of the book consists of case studies, one from the

author's psychoanalytic practice and thirteen from a special study of unmarried mothers at a refuge home. In reading these cases I had a sense of shrinkage from the wide scope of the rest of the book to the constricted dimensions of the consulting room. I did not feel that the reports were combed free of Rorschach clichés and pat psychodynamic formulations, especially those concerning rejection by the mother, and I questioned whether the case material was always adequate to support the conclusions drawn. The cases, to be sure, are not offered in proof of anything; rather, they are intended to illustrate different sources of anxiety and personal patterns of dealing with it, and they have the merit of displaying normal and successful adjustments as well as neurotic ones. Yet even when they are accepted in this light they seemed to me to belong only at the common level of case reporting, not on the high ground that is occupied in the main part of the book.

ROBERT W. WHITE.

Harvard University.

HARSH, C. M., AND SCHRICKEL, H. G. *Personality development and assessment.* New York: Ronald Press Co., 1950. Pp. vii+518. $5.00.

Books on "personality" have many different aims and contents. This recent textbook is intended for a survey or coverage of a wide variety of studies and theories. It aims at being introductory but thorough and to present material in a way that will "minimize theoretical biases . . . [and] . . . aid development of an inclusive and unified perspective toward personality study." The book is organized into two main parts. The first part deals with the nature and development of personality and is organized into chronological periods beginning with infancy and ending with old age. Most of this section (296 pages) deals with generalizations made from experimental investigations and affords an excellent source and organization of a great many studies which might be classified as child development, personality theory, and studies of human learning. This section also includes many specific hypotheses gleaned from clinical observations and theoretical deductions.

The first three chapters in Part II deal with different theoretical orientations to personality. These chapters serve to acquaint the student with some of the broad trends in personality theories, but they do not present a systematic or thorough comparison of personality theories. Compared to the developmental survey portion of the book, this is a less useful section. Although the authors, in general, draw on learning experiments for much of their material, perhaps a major omission here is to fail to give some attention to the beginnings of attempts to approach personality from a "learning theory" point of view as a new but still poorly systematized approach. The remaining chapters in Part II deal with an introduction to personality measurement or assessment and the problem of trait organization.

The authors would be hard pressed to justify as unbiased the selection of concepts used, the amount of space devoted to various topics, and the inclusion and exclusion of topics. Surely there is some implicit theory or bias to this book, as in all books, and perhaps it would have been better to make it explicit and then have the opportunity not only to organize data into logical headings but also into systematic constructs. Failure to do this involves the necessity to cover many overlapping concepts (i.e., anxiety, inferiority, guilt, conflict, etc.) as if they were all quite different things. Although the authors are aware of the problem, this book perhaps abets rather than discourages the proliferation of non-operationally defined constructs in this area where investigation and prediction are already relatively difficult.

This book remains, however, a highly readable and informative book which will serve as an excellent text in beginning courses in personality and development and which will be enjoyed by students. It might be well for instructors to supplement the text with some discussion of the need for operational definition and systematic formulation of experimentally testable hypotheses in a field where communication is low and confusion high.

JULIAN B. ROTTER.

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NEWCOMB, THEODORE M. *Social psychology.* New York: Dryden, 1950. Pp. xi+690. $4.50.

This newcomer among social psychology texts consummates the wedding of psychology and sociology in the author's thinking, research, and teaching. Newcomb has been a leader among those who are convinced that psychology and sociology complement one another and who have been striving to bring these disciplines into more intimate relations. His *Social Psychology* should enhance his leadership in this inter-disciplinary group since it appears to do a distinctly better job of relating certain key concepts and principles of the two fields than any previous text.

An introductory text in social psychology should offer the student a synthesis of the field in terms of concepts and principles which reflect a consistent theoretical viewpoint. Newcomb's text embodies this virtue. Social psychology is defined as the study of individuals interacting, that is, perceiving and responding to each other's responses. Sociology (or cultural anthropology) enters the picture inasmuch as individuals usually interact in structured group contexts where they are under constraint to perform their prescribed *roles* in compliance with group *norms*. The bulk of this text is devoted to showing how (or how profoundly) group roles and norms affect the individual's personality and his interaction with others. To this end, Newcomb marshals psychological, sociological, and anthropological research findings with

such vigor and emphasis that the student can hardly fail to accept his central thesis. Students will probably be especially impressed by his use of anthropological data to relate culture and personality.

Throughout the book, the stress is on the individual (albeit as group member), and the theory presented is largely psychological theory. Much of Newcomb's theory is drawn from general psychology, in particular the terms and principles of motivation, learning, and perception. Adding the adjective "social" to these terms does not call for new theoretical formulae; rather, the same formulae are now filled with items relevant to social interaction instead of such items as maze pathways and pathway choices. Other important segments of Newcomb's approach derive from personality theory, notably his consideration of personality conflict and ego psychology. Of particular interest is his well-reasoned account of the ways in which Freud's and Horney's theories contribute to an understanding of the socialized personality.

Newcomb makes socialization the primary focus of his analysis. Socialization is described as a process in which the initiate learns to satisfy his *drives* or *motives* (organic and socially acquired) by developing *motive patterns* and *attitudes* which correspond to the roles and norms of the groups in which he holds membership. A key term throughout the book is *motive pattern*, which is defined as an organization of the perceptive, cognitive, affective, and performance aspects of one's responses toward a particular goal. In other words, perceiving, thinking, feeling, and doing are viewed as significant in relation to the motives which organize them and which they serve. An *attitude* is defined as a predisposition to have a motive toward its object, and *role patterns* are viewed as organized in terms of the motives they express.

While social motives, motive patterns, attitudes, frames of reference, and role patterns are discussed as learned, Newcomb nowhere offers a detailed analysis of the social learning process. Thus, he uses six pages to discuss how motives are acquired (121–127). He reminds the reader of the trial-and-error and conditioning formulations and posits the acquisition of second-order drives as bases for elaborating motive patterns. For further details of the learning process, he refers the reader to other sources rather than himself testing the applicability of various learning principles to social motivation. Learning new motive patterns is discussed as an aspect of personality formation. In this account, new motive patterns are said to develop when others are blocked. Thus motives will out, though little is said about how new channels of expression are selected.

In outlining the processes of attitude formation, Newcomb uses four pages to assert that attitudes develop out of specific acquired drives, become differentiated, and organize into inclusive hierarchical systems (127–131). He justifies this scant development of the topic by pointing out how sparse are the research findings on attitude formation

in childhood. Attitude change, on the other hand, is fairly well outlined by research findings, and Newcomb devotes the bulk of two chapters to this subject. This account is largely in terms of perceptual reorganization rather than learning theory, with emphasis placed on changes in socially conditioned frames of reference.

One of the more difficult decisions confronting the writer of a social psychology text is how fully to discuss personality structure. Newcomb centers his discussion on those aspects of personality which are most evidently affected by social influences. He recognizes the role of constitutional factors in personality organization and attributes "temperamental" or "expressive" traits to them. Legitimately, he elects not to discuss these factors, limiting his account to "directive" factors such as motives, motive patterns, self-other attitudes, and frames of reference, all of which are discussed primarily in their relations to group influences. He devotes considerable attention to personality dynamics, particularly in relation to the concept of "self," the hierarchical structuring of attitudes, and the interplay of motives, perceptions, and attitudes.

Individual similarities and differences in personality as revealed in attitudes and social behavior offer an important test of Newcomb's account of personality in terms of group influences. He passes this test well by accounting for similarities in terms of common group experiences, differences in terms of distinctive group experiences. The uniqueness of individual personality is easily accounted for by pointing to the unique combinations of social influences with which each individual's multiple group memberships supply him. An important contribution of Newcomb's account of personality in society is his discussion of ways in which personality conflict is related to conflicting roles and norms in the various groups to which the individual belongs.

If one looks for the contributions of sociological theory to Newcomb's viewpoint, he finds essentially an analysis of group structure in terms of interlocking roles and group norms (Chap. 8). It is hard to find a page in the book where the discussion does not relate to roles or norms. However, Newcomb makes clear that these group factors affect individuals only through interaction with other individuals. And so the theory called for in social psychology is primarily a theory of the interaction of individuals in face-to-face groups. As Newcomb points out, the interaction of individuals is the meeting-ground of individual psychology and sociology. Appropriately, sociologists (such as Bales with his "interaction process analysis") and psychologists are now working jointly toward developing interaction theory.

To this reviewer, the greatest missed opportunity in Newcomb's text is his failure to venture a specific and integrated account of interaction. This may seem an odd complaint since he builds his entire presentation around analyzing the effects on the individual of his interaction

with others, as well as discussing ways in which the individual may influence the groups in which he performs roles. However, Newcomb rejects the task of classifying and analyzing various forms of interaction such as helping, cooperating, attacking, and loving. Rather, he concludes that "the major social-psychological problem . . . is not that of listing and classifying the possible forms but of discovering the *conditions* under which an identifiable kind of interaction does or does not take place" (p. 24). By and large, it seems fair to say that Newcomb deals more with the effects of group roles and norms on individuals than with the processes of interaction which accomplish those effects.

What sort of interaction theory is now proposed? This reviewer's suggestion is that interaction theory be developed on the premise that the simplest and most fundamental instances of interaction involve two individuals (the "dyadic group"). Viewed in their group context, the two would be seen as performing mutual roles which defined the modes of their interaction. Each would be viewed as bringing to the interaction his own motives, attitudes, and frames of reference. Each segment (episode) of their interaction would be interpreted or evaluated in terms of how it affected their perceptions, attitudes, and motives and how it affected the course and products of their interaction. Presumably, the same theory would apply to larger groups, though additional terms and principles would probably be needed to account for the sub-groupings of members. Newcomb has certainly pointed to the need for such a theory as the one proposed, and has offered many valuable leads toward its development. It is a matter for regret that he has not assembled his ideas into a more systematic account of interaction.

Newcomb's text strongly represents the scientific approach. He defines his terms operationally and points to the necessity for intervening variables, such as motive, which must be measured indirectly. He stresses theory and makes excellent use of research findings to illustrate or test the hypotheses he advances. He shows the utility of his theory for working out solutions to practical problems such as overcoming prejudices and reducing group conflicts.

The text would have been strengthened by a discussion of observational methods and by a consideration of problems encountered when designing and conducting social-psychological research in the laboratory and in "real-life" situations. Some discussion of the relations between basic and applied research would have helped the student understand the significance of current research trends in the field.

Viewed as literature, this text is a superior job of exposition. Illustrative materials, drawn largely from research studies, are well chosen for scope, relevance, and interest value. Each chapter begins with a section calculated to evoke the student's interest, followed by a section stating the chapter objectives. Summaries are frequent, and highly effective use is made of parenthetic remarks to answer on-the-spot

questions which students are apt to raise. Probably the tables would be improved by supplying titles, and more consistent use might be made of italics in defining terms. Some of the terms used, such as "dependable motive" and "marginal man," may prove more confusing than helpful to this developing science.

An outstanding feature in the development of this text as a teaching instrument is the fact that a mimeographed edition was used and a research evaluation (to be published) made on the basis of student reactions. When will the day come for supplanting the primitive form of professional evaluation represented by this review with a research evaluation based on reactions of a sample of instructors who might use (or have used) the book as a text?

All in all, Newcomb's *Social Psychology* appears to be an able and valuable synthesis of concepts and principles drawn from general psychology, the psychology of personality, sociology, and cultural anthropology. It is a dignified and scholarly work which reflects the exacting requirements of science. Yet it is clear, vivid, and interesting throughout. Here, then, is a text well suited to win the strong endorsement of scientist, instructor, and student alike.

GLEN HEATHERS.

The Fels Research Institute.

ALLPORT, G. W. *The individual and his religion.* New York: Macmillan, 1950. Pp. viii+147. $2.50.

It may be just a coincidence, but Allport's title is reminiscent of *The Individual and His Own*, curiously enough, perhaps the most iconoclastic work produced in the nineteenth century. Max Stirner's (i.e., Kaspar Schmidt's) formidable book was more appealing to the youth of the latter half of the century than the Kinsey Report is to the present generation. Allport's title, however, has a sound reason per se, the idea behind it being the fact that religion is an individual experience and cannot be treated *en gros*. It is evident that the author is not concerned with dogmatic or organized religion but with the sentiment as such.

In his search for a definition of the religious sentiment, Allport appears to be partial to the German writers, Schleiermacher, Otto, Wobbermin, Max Weber. Yet it was in France that the psychology of religion had its beginning, and it was largely the French influence which gave impetus to William James. It is perhaps an oversight that Allport has not so much as mentioned the Franco-Jewish social philosopher, Émile Durkheim, whose large volume on religion has been a stand-by for a whole generation of sociologists, not only in the French-speaking countries. Yet we can hardly expect a complete Baedeker within the compass of restricted travel; and the author has been highly discriminating in his selection of opinion before concluding that "there

are as many varieties of religious experience as there are religiously inclined mortals upon the earth"—a conclusion which will draw cavils from many quarters.

Although much of the material reviewed in the chapter on the religion of youth had been covered before in a collaborated article, the statistical account is relevant and interesting. But the results are not as encouraging as the religionist might wish to see them. In the first place, perhaps the chapter should be headed "The Religion of Social-Relations Students at Harvard and Radcliffe." Even then I am not certain whether the figures would afford us a clear-cut picture. The summation of results gained from Catholic and liberal Christian or Jewish students reminds us of the averaging of wages and salaries received by employees and high executives in General Motors.

Allport (p. 43) is at pains to gloss over the inconsistency between most of the students' believing in God of some kind and yet ruling out the supernatural. He thinks that it shows dissatisfaction with traditional ecclesiastic positions, but why are the Catholic students satisfied with their traditions? Is it not a function of (a) the kind of upbringing, (b) the degree of reflectiveness, and (c) the index of emotionality? If we had full data on these three factors, we might be able to predict the religious status of the particular student.

Has Allport asked himself what a similar questionnaire in a Moscow or Leningrad university would show, or for that matter in a Moslem institution? To my mind, it is plain that the majority of those steeped in religious rites would remain religious while a few bent on questioning everything would break away from religion, whereas those receiving a wishy-washy or lukewarm religious education would not be interested one way or another until some crisis perhaps brought out some impulse connected with self-preservation.

There is much to say on the efficacy or futility of prayer, which Allport introduces as a function in religion, but it is difficult to accept the statement that "prayer is continuous with hope as hope is continuous with prayer." Hope and fear are scarcely rectilinear but are to be envisaged in a see-saw relation. Prayer is only the *practical* resort, in either case, when the person affected is religious, not otherwise. We can hardly imagine a Tom Paine, an Ingersoll, or even a Jefferson praying when in distress.

In spite of its small compass (less than 150 pages), the book is chockfull of information and reflections set down in the best tradition of the essay. There is skillful brickwork in the rearing of the whole structure, overlaid at many points with significant utterances from the thinkers who had something to say. The elegant diction is reminiscent of the style of William James, and the whole book is richly embroidered with illustrations, which makes the little volume an example of clear exposition. The essay, too gracefully flowing to be a closely-reasoned dis-

quisition, is a forthright examination of the whole religious situation in a psychological light. At no time is there any attempt made to by-pass the knotty problems which present themselves to the serious student.

A. A. ROBACK.

Cambridge, Massachusetts.

MUNN, N. L. *Handbook of psychological research on the rat.* New York: Houghton Mifflin, 1950. Pp. xxvi+598. $7.50.

There is a mere handful of psychologists who can give us a whole series of outstanding books. Professor Munn is one of them. This is his fourth book, and he has done it again—a monumental and magnificent job! It is scholarly, exceedingly thorough, well written, well illustrated, and very timely.

In a way this is a revision of Professor Munn's earlier book (1933), *An Introduction to Animal Psychology.* That one was about the rat, and this one is too. The first book, however, had only six hundred references, and this one has twenty-five hundred—an increase of four-fold—which is virtually all of the articles that have ever been written about the rat as an experimental subject. So Professor Munn had to make a difficult choice, whether to write a textbook based on a selected minority of research or a handbook that somehow covered them all. Fortunately for psychologists—and their students, too, I think—he chose to do the handbook. The result is an amazingly complete coverage of research on the behavior of rats.

The main text of the book is 480 pages of double-column print. It is organized into 10 chapters and 101 major subdivisions. A very attractive and well-designed table of contents lists all the divisions, major and minor, and thus makes the book much more usable. There are 180 illustrations and 15 tables. These have been selected and presented with care to show major methods and results of research. It is helpful in this kind of book that they are also listed in the front matter. Following the main text is an appendix listing more than fifty books in comparative psychology, another appendix giving many additional references to research with other vertebrate and invertebrate animals, and finally an appendix giving about twenty-five hundred references to rat behavior—a really comprehensive bibliography. The author and the publishers are to be commended especially for the very attractive way in which the book has been prepared and manufactured.

As a handbook, it will be extremely valuable for all psychologists doing research, or interested in research, with rats. Its main divisions are: (1) "Introduction" (including the care and handling of rats, and research procedures), (2) "Unlearned Behavior," (3) "General Activity," (4) "Motive, Emotions and Hoarding," (5) "Sensory Processes," (6) "The Role of Sensory Processes in Maze Behavior," (7) "The

Learning Process," (8) "Some Aspects and Conditions of Learning," (9) "Systematic Psychology," and (10) "Abnormal and Social Behavior." Under these main divisions, psychologists will find topic after topic reviewed in such a scholarly and coherent manner as they can find no other place in the psychological literature. Physiological psychologists will find details and illustrations of research otherwise found only in the original literature. Systematic and learning psychologists will find a stimulating and up-to-date account of their controversies and the experimental evidence pro and con. Even clinical and social psychologists will want to consult it for the many experiments with the rat that now bear on their problems.

It is worth emphasizing, I believe, that this book is not merely a compendium of researches. It gives coherent stories—at least as coherent as the status of research will allow. And it is to a certain degree critical, pointing out some of the shortcomings and strong points of various studies. More than that, there are some ideas in the book—ideas for further research and for the reinterpretation of past research—but the ideas are labelled clearly enough to let the reader not mistake them for facts. For all its thoroughness, then, this is not just a fact-grubbing handbook, but an integrated, critical, balanced, and interpreted account of psychological research as it is represented in the rat. It is a book, I believe, that no psychologist can afford to be without.

Even though it is a handbook, it is perhaps unfortunate that it is called one, for that may deter instructors and students from using it as a textbook. It is, indeed, an outstanding textbook. The style of writing, although it is not as elementary as in good introductory texts, is clear and interesting. Its illustrations are well chosen to represent the most significant materials.

It does, however, have some drawbacks as a textbook. One is that it is a little long for a short course in comparative psychology. Because it is so well organized, however, an instructor can select parts to leave out if he needs to cut down reading assignments. A second fault, as a textbook, is its complete coverage of the literature. References and names sometimes come along at a tremendous pace—more than the typical student wants to take. On the other hand, the writing and the continuity of the text are so good that students, with proper guidance from the instructor, can learn to skip the rapid-fire of names at the points where they occur. Thirdly, the book is exclusively about the rat, and I agree with Beach that comparative psychology *should* be evolutionary in outlook, not confined to a single species. The fact is, however, that the vast majority of our animal research is with rats and that the rat is therefore our main introduction to animal behavior. It is better to have a good book on the rat than a poor one about a lot of animals. Moreover, Munn has rendered the additional service (in an appendix) of giving a well-chosen list of references about other animals

of the phyletic series, and the instructor can supplement his lectures and reading assignments from these other materials.

All in all then, although it is a handbook, it is also an excellent textbook, and I hope teachers will consider it for their courses in comparative and animal psychology. It might also serve very well in courses in advanced general psychology.

Your reviewer is so impressed with this book that he feels he can not do it justice in this brief space. Moreover, what minor faults it has do not deserve to be mentioned alongside its exceptional merits. Few, if any, authors could do what Dr. Munn has done, and none has! He has rendered an outstanding service to his colleagues, for which they certainly will be grateful. Many a student will thank him too.

C. T. MORGAN.

The Johns Hopkins University.

KATZ, DAVID. *Gestalt psychology: its nature and significance.* (2nd Ed., English trans. by Robert Tyson.) New York: Ronald Press, 1950. Pp. x+175. $3.00.

This book is, with certain exceptions, a series of 29 précis about the concepts, principles, and theoretical applications of Gestalt psychology. Katz writes clearly, succinctly, and with a mature perspective, and his translator has put his thought into equally happy English. The first half of the volume—the first 16 of his neat, well-integrated, little chapters—gives as clear, as intelligible, and as unbiased a view of Gestalt psychology as can be found today. The treatment is more extended than the histories and system-books provide, and it is less technical, less argumentative, and less interrupted by references to personal opinions of the great *Gestaltpsychologen* than are the more technical accounts which quote the literature and cite the details of research. Katz is persuasively dogmatic. He mentions particular persons and researches only casually, confining himself to what seem to him and to this reader the incontrovertible facts which have passed from the realm of personalization to the common domain of science. It is refreshing to read that kind of sure discussion in psychology.

The chapters are truly short. Nine of the 29 chapters are three pages long or less, and 24 of them are seven pages or less. These pages are, moreover, little pages—about four hundred words each. Any journalist knows that reading is made easier when subdivision is frequent and clear, and Katz himself in his discussion of mental work in this book shows that efficiency is greater when the total task is clearly separated into smaller parts. Here, however, we have no caption-writer's artificial separations, for Katz has aimed each précis at a specific goal of understanding. The reader is not left in suspense by unresolved argument nor crowded into acceptance because his doubts and reservations have become too many to remember. The author teaches him comfortably

and wisely, point by point, and this reviewer profited from the instruction he received.

Katz writes from forty years' experience in the periphery of Gestalt psychology. When Wertheimer was bringing the new Gestalt psychology to birth at Frankfurt in 1910–1912, Katz was in G. E. Müller's laboratory in Göttingen working on the phenomenology of color, and he published a year before Wertheimer. So we see that the phenomenological revolt against Wundtian atomism was already in the air, an event ordained in the evolution of the *Zeitgeist*, a piece of progress for which Wertheimer's brilliance may have been a sufficient cause but hardly a necessary one. Faithfully and appreciatively Katz expounds the Wertheimer-Köhler-Koffka principles, regrets that these three have so often limited their discussion to the phenomena of vision, adds from his own researches and general wisdom the proper examples and evidence in the fields of touch, hearing, and action, and thus succeeds, by mixing appreciation with criticism, in being convincing without the appearance of special pleading.

This review is not the place to repeat the standard arguments for phenomenology and against atomism, for field theory and against production theory, for the immediate cybernetic adjustment of the organism to new conditions in accordance with physical laws and against the slow adjustment by conscious deliberation or unconscious inference. Katz has dealt well and clearly with all of these matters and more, and the reader must go to his text to hear what he says.

Katz exhibits Gestalt theory as a kind of field theory, and in this matter we become aware of another way in which Gestalt psychology was borne in upon the tide of the *Zeitgeist*. Field theory is a reaction against Newtonian mechanics and action at a distance. In physics the historical line for it is Faraday–Maxwell–Planck–Einstein. Einstein was discovering Maxwell at the turn of the century and published his special relativity theory in 1905 when he was twenty-six. Planck was at Berlin. Later Einstein, Köhler, and Wertheimer were all at Berlin. Köhler says that Planck's was the mind which influenced him most in his predoctoral days. Wertheimer knew Einstein and admired him. Gestalt psychology is a kind of relativity theory, a psychology of field patterns. Katz does not say all this, but anyone who will read his book together with P. A. Schilpp's *Albert Einstein: Philosoper-Scientist* (1949) will at once see the significance of the latter for the former.

In the second half of this book, when the discussion has passed beyond the integrations of perception and of action to the general topics of thinking, memory, volition, and emotion and to the selected applications of Gestalt theory in medical, comparative, child, and social psychology, the exposition becomes less satisfying. In part the fault here lies with psychology and not with the author. These topics have not been advanced by Gestalt principles so much as has the business of perception and motor integration. The new concepts clarify certain

topics within these fields but do not reorient them, not if Katz's words about them are all there is to say. Especially in the final chapters does his selection seem arbitrary and not rounded out into the perfect précis that were his first chapters. To write the title "Medical Psychology" and then to discuss nothing but perception of form in hemianopsia, is to reduce this part of the discussion to mere incidental comment on a pertinent but adventitious topic.

Presumably the author is less at home in these later chapters, and thus the reader feels that he is being less expertly led. Let me give one example. Katz objects to associationism as dealing with separate connections and being atomistic. He gives many instances (chaps. 20–22) to show how the total constellation of the task determines learning, the formation of new mental structures. In other contexts he cites Hering and Ehrenfels as having anticipated the Gestalt psychologists in recognizing the importance of the influence of a whole field upon its parts. Why should he not, therefore, mention Ebbinghaus here instead of leaving the reader to think that nineteenth-century connectionism was thoroughly atomistic? Is there any law of Ebbinghaus' in his *Ueber das Gedächtnis* of 1885 that does not show how the total task overrules the simple law of frequency of contiguity? That it takes more repetitions to learn a longer series is a fact comparable to Hering's noting that in visual contrast the retina acts as a whole.

The other difficulty with the latter part of the book (chaps. 21–23) lies in Katz's departure from his rule of presenting only facts and principles while he omits detailed discussion of the supporting research. In these chapters he launches into description of certain experiments from his own laboratory, studies of mental work, connective inhibition, mental dazzle, and the transposition of action forms. ("Mental dazzle" is the confusion that arises when instruction or information is given too rapidly for immediate comprehension, the state from which Katz so ably protects the readers of his book.) These experiments are original, interesting, germane each to its topic, but so are the many other experiments which the author cites, some of them now become classical in Gestalt literature. A book is not a suitable place for the description of experimental techniques nor for the presentation of the raw data from which conclusions are drawn. Katz gives these results in new chapters which were added in this second edition, and in doing so he spoils the artistry of his book. If the journal sources for these researches are not easily accessible to the readers of the discussion (and they seem not to be), then the experiments could be described in appendices with only their conclusions cited in the text like the conclusions of all of the other pieces of experimental work.

The book was published first in Swedish in 1942, then in German in 1944. The second edition, radically revised all through and with four new chapters added (chaps. 13, 21–23), came out in German in 1948, and now in English with a preface dated March, 1949. The publisher has

matched the author and translator in good work (though Fig. 31 is upside down), and there is a short bibliography and two indices. The text will make excellent reading for undergraduate seniors and first-year graduates in psychology in their systematic courses. Katz, at the periphery of Gestalt psychology, can see the whole of it more clearly and with less ego-involvement than those at its center, and he is thus a proper first-instructor for the novice, who can advance later to the large literature of dissent and special pleading.

EDWIN G. BORING.

Harvard University.

PROTHRO, E. TERRY, AND TESKA, P. T. *Psychology—a biosocial study of behavior.* New York: Ginn & Co., 1950. Pp. ix+546. $3.75.

This book is intended as a text for the beginning course, and its announced thesis is "that a human individual is a biological organism living in a social environment, and that it is therefore necessary, if we are to understand the behavior of human beings, to study the nature of biological organisms and the nature of their social environment." Many instructors and probably most students will, however, wish that there were relatively more study of human behavior itself and less of biological structure.

Part I contains the two conventional chapters on what psychology is and on scientific method, plus a third which might better be entitled "The Concept of Self." Although this third chapter is not a philosophical discussion of the nature of the self, it is abstract and generalized to an extent that makes one wonder what has been learned from the reading of it. One point seems to be that "everyone has a highly distorted idea of what he is like," and "this distortion is almost invariably in the direction of overestimation." Consequently, "when a more objective examination is made of the individual—the interest in investigating oneself is likely to evaporate." Even were all these statements established by scientific procedures as laid down in the preceding chapter, one wonders about their effect in motivating the beginning student.

Part II consists of four chapters on the dynamic basis of behavior, including two describing the social environment as it creates and channelizes motives. The reviewer is admittedly muddled in the field of motivation, always wondering why it is better to refer to a gregarious motive than to describe common patterns of behavior. But if motives are to be discussed at any length, then the role of society in creating and channelizing them is highly pertinent. This socio-psychological treatment of motivation is a highly commendable feature of the book.

The reviewer objects to Part III, devoted to the structural basis of behavior, on two scores, but what causes him to object will cause some to commend. First, it is too lengthy and too detailed. It consumes 156 pages as compared to 158 pages devoted to behavior itself. Second, it

is too unrelated to behavior, too much sheer description of structure without reference to functioning. Chapter 9 is devoted to organs of adjustment in the lower animals, a condensed course in comparative anatomy. The chapter on heredity might just as well be found in any biological text, for practically the only reference to human heredity is a short treatment of its complexity and a concluding statement that "from our study of the principles governing the transmission of traits, we can see that man's structure is largely inherited and that certain limits are set for him by the kind and quality of organism he inherits."

The five chapters of Part IV dealing with integrated behavior are "Observing," "Learning," "Efficient Learning in College," "Intelligence," and "Vocational Guidance." The logic of placing "Vocational Guidance" under a section on integrated behavior is a bit puzzling. The absence of chapters on thinking and emotions is noticeable, and some will miss the present-day treatment of personality.

The book as a whole is well written and adequately documented. The authors have carried through their intended stress on biological and social factors. Those teachers of the beginning course who emphasize these factors in their presentation will certainly want to consider adoption of this text.

C. N. REXROAD.

Stephens College.

KLINEBERG, OTTO. *Tensions affecting international understanding.* New York: Social Science Research Council, Bulletin 62, 1950. Pp. xi+227. $1.75.

Klineberg's survey of research on tensions affecting international understanding is treated in four parts: (1) personality in relation to nationality, (2) national stereotypes, (3) attitudes and their modification, and (4) influences making for aggression. The first of these topics is examined with full reference to the relevant literature. The latter three are illustrated by a sampling of representative studies. This monograph cannot serve, therefore, as a bibilography, but it is most unlikely that any of the conclusions drawn from this survey would suffer substantial modification by any research omitted from consideration.

The conclusion one must draw from this monograph is that the social sciences are ill-prepared to deal with international tensions either conceptually or pragmatically. In each of the four areas investigated by Klineberg there is a paucity of material that can be made to add up unequivocally to any general conclusion. One seeks in vain for a hard base of interrelated data or systematic theory upon which to build any programs designed to improve materially international understanding.

The following more specific conclusions seem to be warranted: (1) No empirically-based descriptions of national differences in personality have been made with sufficient reliability to produce substantial agree-

ment among investigators. (2) Although a variety of ingenious techniques have been employed in the study of national character, there is yet to be reported a coordinated study of the same nation using these various techniques. (3) There exist no strictly comparable studies of national stereotypes from different countries. (4) The bulk of the work on modifying attitudes has been concerned with intergroup relations within a country, with little study of the modification of attitudes toward other countries. (5) At the present time it is most difficult to make specific suggestions to an international organization interested in utilizing the best methods for improving international relations. (6) Research on attitude change has been largely symptomatic and unable to deal effectively with causes. (7) Facilities for conducting international research are meagre, but those that do exist are being poorly used. Even where it is possible to conduct comparable studies in different countries, the opportunity is seldom seized. (8) Although a good deal is known about some of the influences making for aggression in the individual, the relations, if any, between individual aggressiveness and national aggression are virtually unknown.

Klineberg's monograph should serve to stress the importance of more and better organized research in this field. It should indicate what approaches have appeared to be promising and what unrewarding. Since, however, the conclusions he can draw are negative, the suspicion cannot be readily put down that the whole approach to the problems has not yet been phrased right. Proper problems for research have not yet been stated. What is now needed is an imaginative and positive proposal for a conceptually-integrated program of research that can make use of existing facilities and stimulate the creation of new ones.

DORWIN CARTWRIGHT.

University of Michigan.

ZAHL, PAUL A. (Ed.) *Blindness: modern approaches to the unseen environment.* Princeton: Princeton Univ. Press, 1950. Pp. xvi+576. $7.50.

CHEVIGNY, HECTOR, & BRAVERMAN, SYDELL. *The adjustment of the blind.* New Haven: Yale Univ. Press, 1950. Pp. xvi+320. $4.00.

Vision is usually considered the sense modality with greatest significance for human adjustment to the environment. Because of this significance the loss of vision in blindness presents a wide variety of serious adjustmental problems. Further, the status of the blind in the social community, as revealed in earliest written records and as easily observed in modern culture, creates even greater difficulties for their adjustment. The two books being reviewed are important discussions of the adjustmental problems of the blind.

The volume edited by Dr. Zahl is encyclopedic in scope. While its origin is found in the activities of the Committee on Sensory Devices of the National Research Council, the final contents of the volume

discuss a wide variety of problems of blindness. Thirty-one contributors, of whom ten are themselves blind, have contributed thirty-three chapters divided into seven parts: History and Welfare, Education and Psychology, Vocational Considerations, The Military Blinded, Time-Tested Boons to the Blind, Exploration of New Technological Horizons, and Ophthalmological Factors. Of specific psychological interest are chapters by Kathryn E. Maxfield on "The Preschool Blind Child"; Berthold Lowenfeld, "Psychological Foundation of Special Methods in Teaching Blind Children"; Samuel P. Hayes, "Measuring the Intelligence of the Blind"; Thomas D. Cutsforth, "Personality and Social Adjustment Among the Blind"; Edward A. Jerome and Harold Proshansky, "Factors in the Assay and Use of Guidance Devices"; and Karl S. Lashley, "Psychological Problems in the Development of Instrumental Aids for the Blind." These chapters are selected for mention because of their direct psychological content. However, the material concerned with the extent and care of the blind in the United States and other countries, problems of educational and vocational rehabilitation, methods of communication and travel aids, and the modern engineering efforts to develop electronic devices as aids in obstacle detection and in reading are of interest in considering the many problems of adjustment.

The volume by Chevigny and Braverman is a more personalized account of the adjustmental problems which face the blind. The senior author, who is himself blind, has collaborated with a clinical psychologist to explore the problems of blindness, especially as they relate to the usual social attitude toward this handicap.

In both of these volumes and especially in the second one there is evident a hope, or perhaps a demand, that the blind be given an opportunity to compete within their abilities for roles in our society. Historically the blind have been considered rather hopeless objects of charity. Chevigny in particular protests this status, and there is abundant evidence in both volumes that, given opportunity for individual activity, the blind will show achievement in many directions comparable to that of the sighted. While the problem of blindness has significant sociological and educational facets, the fundamental problems are psychological both in the areas of perception and of personality adjustment. Both books will of course be read with profit by all persons working with the blind, but they should not be neglected by psychologists working with any handicapped group, and they have much of value for clinical psychologists in general.

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GREENBLATT, M., ARNOT, R., AND SOLOMON, H. C. (Eds.) *Studies in lobotomy.* New York: Grune & Stratton, 1950. Pp. x+495. $10.00.

This book of 25 chapters summarizes the experience which was

gained at the Boston Psychopathic Hospital since 1944 using translateral lobotomy on more than five hundred mental patients. The various chapters were written by different investigators and vary in their completeness and adequacy. Chapters 1, 2, 3, 4, and 11 introduce the problems and review the literature. The bibliography is very extensive. Chapters 5 and 6 are on postoperative behavior and nursing problems.

Of particular value and significance to psychologists are chapters 7, 8, 9, 10, and 12, all of which deal with various aspects of an extensive follow-up of 205 cases. Chapters 14 through 24 deal with special studies or reports from the standpoints of anatomy, cerebrospinal fluid, autonomic nervous system, bladder control, skin temperature, pain, electroencephalography, sociometric measures, and occupational therapy. Chapter 22, by C. R. Atwell, reports psychometric results on nine patients tested before and after operation with the Rorschach, sorting tests, and the Shipley test. Because of the paucity of patient material suitable for psychometric study, little was elicited by these methods.

The follow-up studies are the first of their sort which are based on a large enough group of patients so that one may have confidence in the information, which was badly needed. The report of the one- to four-year follow-up of 205 cases is by Arnot, Talbot, and Greenblatt. Social problems during the first year at home in 35 cases are reported by Broffman. Sexual behavior was investigated by Levine and Albert. The follow-up study of Arnot *et al.* made use of rating scale techniques to evaluate 30 different factors. They used the scales only when relevant before-and-after information was available. The most important change produced was the elimination of "tortured selfconcern" (fear, worry, and depression) in 87 per cent of patients studied. Of 141 patients rated on initiative the tendency was 1.6 to 1 in favor of a lower rating than had marked these patients before operation. It is pointed out that this decrease (laziness) is most marked during the first half-year, after which there is a tendency for increased initiative to appear. However, both increased age and schizoid apathy tend in many cases to fix the lost initiative at a low level.

Auditory hallucinations disappeared in 27 per cent of the patients. An improved appetite was shown by *all* patients after operation. Lack of cooperation, distrustfulness, resistiveness, and combativeness were all sharply reduced while irritability and outspokenness were increased. The vocational or work adjustment varied. In general this adjustment was not on as high a level as before the onset of illness.

Of 77 psychosurgery patients out of the hospital and living in the community, 45 were of good mental status, 23 of fair status, and 9 of poor status; of 104 patients still in the hospital, 4 were of good mental status, 27 of fair, and 73 of poor status.

"Despite many favorable signs, however, the relatives often complained, for the presence of even one unfortunate postoperative symp-

tom in an otherwise symptom-free person could at times cripple the group activities and spoil the freedom and happiness of the entire family." The lobotomized patients in the community were not more "negative" members of society than they had been before their illness. These persons had never contributed their share to the community's activities before illness, and even among cases with best results from lobotomy many were still minus quantities. It is not the degree of assumption of financial responsibility by the patient that is disturbing so much as it is the unproductive or unoccupied attitudes.

Other traits or characteristics which have been mentioned as appearing after operation and hence probably attributable to lobotomy are alcoholic excesses, restlessness, irritability, poor appearance, overtalkativeness, deficiencies in insight, and lack of ability to do calculations. Of these only greater irritability and decreased ability to do calculations were actually increased in frequency of appearance. Other traits in the list were more often "normalized" than "abnormalized." The only one of these traits complained of by the patients was the feeling of increased irritability. In general, patients and relatives reported that preoperative moral, social, and religious attitudes continued to operate after lobotomy although many patients reported that there was a reduction in guilt feelings, modesty, and embarrassment in association with sexual activity.

Based on this survey the investigators drew up a profile or description of the "ideal" patient most likely to respond favorably to lobotomy. (Starred items below are of special importance.) Such a patient would be married, have children, be a college graduate, Protestant, hospitalized less than two years and sick less than four years after having had previous remissions from attacks of mental illness, free of organic brain damage, rapid onset,* had had electric shock therapy, had been sociable, not particularly irritable, of good family, social, and vocational adjustment. During his present illness he should have had no auditory hallucinations,* retained some insight,* had no delusions or ideas of reference, been depressed or elated but not apathetic, and have had some compulsive-obesssive* symptoms.

Drs. Greenblatt and Solomon write the summary (chap. 25) which is as fair a statement of our present-day knowledge concerning lobotomy as this reviewer has seen. This volume is recommended reading for all interested in psychosurgery and psychopathology.

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METTLER, F. A. (Ed.) (The Columbia Greystone Associates.) *Selective partial ablation of the frontal cortex.* New York: Paul B. Hoeber, 1949. Pp. xiii+517. $12.50.

This book has been hailed as a monumental work in the compara-

tively new psychiatric field of psychosurgery. There is no doubt that it is a fine example of cooperative research in which disciplines of psychiatry, psychology, and neurosurgery have for once let down their barriers and joined forces to consider a question which has been asked many times, namely, "What are the effects, if any, upon a patient who undergoes partial ablation of the frontal cortex?"

For practically the first time in psychosurgical research, a control group was used. However, the reviewer feels that the method of selecting the control group and the experimental group (24 cases in each group) is open to question.

The method used in selecting control and experimental groups involved a matching technique in which two lists were drawn from a master list which included age, sex, marital condition, IQ, occupation, final preoperative diagnosis, previous EST, insulin, metrazol, length of hospitalization, and previous admissions. "The judgment as to which one of the pair of matched patients was to be operated upon was made on the basis of medico-surgical and prognostic considerations, evidence of spontaneous improvement and temporary unwillingness on the part of the patient to be operated upon" (p. 21). The reviewer thinks that a better method of securing a control group is now being used in a study conducted by the staff of the Roseburg (Oregon) Veterans Administration Hospital. In this study every other or every third patient in a group of fifty will be given a pseudo-lobotomy, i.e., bone buttons are removed and replaced without completing the operation. The "two-stage operation" patient then will be given identical treatment to the "full stage" patient. He is studied for four months and at the end of this period, if indicated, the pseudo-lobotomized patient will undergo a complete lobotomy and hence will act partly as his own control.

A second question may be raised about the treatment of the control group. In the Greystone study, the control group was anaesthetized with sodium pentothal and 500 cc. of blood was taken. Theoretically this would equate two groups for blood loss but the question of knowing which group had the topectomy could hardly be disguised. This point is important since, in spite of instructions on the part of the medical staff to treat each group in the same manner, there is a good possibility that the two groups received different treatment.

Aside from this possible criticism of the experimental design, the postoperative treatment might have been changed also. This was mentioned by the experimenters; and the lack of staff for instigation of group and individual therapy was given as the reason for neglecting this aspect of treatment. The reviewer still feels that at least part of the recovery attributed to psychosurgery is due to the "total-push" type of therapy which a topectomy or a lobotomy usually brings to the patient, if for no other reason than the fact that he is receiving special attention.

Chapter 2 deals with prognostic expectation of patients under ordinary institutional care. This chapter is of great interest since it brings up the question as to spontaneous recovery in patients with psychoses of approximately the same severity as those which the patients in the control and experimental groups had. The authors state, "Bearing these facts in mind, it can be assumed with reasonable certainty that any improvement shown in the series of patients treated surgically was due to topectomy operation" (p. 29). The reviewer feels that this statement is somewhat optimistic since "total-push" therapy was not tried in the 500 cases on which these findings were based.

Section II of this work, entitled "Medical Survey," describes in detail the laboratory findings and the physical examinations of the patients in the study. As with the remaining chapters of the book, a short summary enables the reader quickly to survey the results. Of more particular interest to psychologists are the findings in this section in the fields of audition and vestibular function. The authors found that topectomy did not influence auditory function directly and was often useful in making uncooperative patients more accessible to audiographic tests.

Chapter IX, dealing with vestibular function, is of particular interest to the experimental psychologist, mainly from the instrumental point of view. The description of the Holsopple chair, used to obtain cinematic records of the nystagmus reaction, will help other experimental psychologists in dealing with this somewhat neglected aspect of perception. Holsopple has devoted many years to this sensory process and at last has succeeded in overcoming the technical difficulties always present in previous rotational studies, namely that of controlled acceleration. In general, the authors found that topectomy resulted in lengthening of nystagmus time and that there was a positive relationship ($r = +.75$) between the amount of tissue removed and the objective scores. They observe, however, that more extensive testing should be done before this evidence can be accepted as conclusive.

Chapter X, dealing with the visual apparatus, reveals that in the present sample there is no effect of partial ablation of the frontal cortex on visual acuity, acuity of peripheral vision, and color vision. Interestingly, there is some change noted in the autokinetic phenomenon; it appeared to be absent three weeks postoperatively and then recovered three months postoperatively. This recovery is not complete; the patterns are "simpler" after the operation.

Section III is of most particular interest to clinical psychologists. As the author of this section states, "The primary question for which we sought an answer was: 'Does excision of frontal lobe tissue influence behavior and mental life?'" They find, as might be expected, that psychologic and psychometric changes do take place; there was no

real loss of learning, memory, or intellectual function; there was a statistically significant gain in recall and perceptual recognition. Loss of ability to do abstract thinking apparently was not demonstrated. Apparently psychologic changes depended to some extent on the areas involved. Areas 9, 10, and 46 are associated with social recovery. Ablation of Area 11 showed little or no change in behavior. Area 44 removal was not related to changes in verbal or speech function. In reviewing the areas ablated in the Greystone studies, it is interesting to note that the anterior cingulate gyrus (Area 24) was not reached except in one case; A. A. Ward and K. E. Livingston, two neurosurgeons, feel that this area may well be related to behavioral changes. Evidence to date indicates this to be true in monkeys.

The remaining chapters in Section III deal with several new psychologic techniques used for the first time with psychosurgical patients. To measure changes in affectivity, one of the widely-discussed effects of such operations, M. Garrison devised two questionnaires which attempt to measure anxiety and complaints. These questionnaires (30 questions each) were answered by the patient, and responses were weighted. Results were interpreted cautiously and, as the investigators state, represent tendencies only. They found, for example, that anxiety as measured by their questionnaires tended to decrease in patients with Areas 10 and 46 removed. This decrease was related to social improvements. Complaints as measured by the second questionnaire also decreased.

In addition to these inventories, the Mirror Drawing task was used as a measure of affectivity. Using blocking (defined as interruption of tracing for a period of time) and frustration (measured subjectively on a five-point behavior scale), they found a tendency for blocking to decrease when Area 9 was excised with patients of average intelligence. Frustration did not seem to be related to any of the criteria considered.

Radical surgery of the frontal lobes was not reflected in changes in Rorschach protocols; no definite patterns of changes could be observed in the operative group.

Section IV deals with the results of the experiment in terms of psychiatric, neurologic, and pathologic changes as seen before and after topectomy and lobotomy. The psychiatric findings in general are not too encouraging. The experimenters in this section are cautious in making any interpretation, because of the small sample. However, they do state definitely that there was no relationship between weight of brain removed and improvement as measured by behavior scales, and that therapeutic response did not seem to be related to type of onset of illness or precipitating causes. Favorable response to previous electro-shock therapy (EST) tended to be predictive of good results of the operation. Lastly they found that clinical improvement was best in younger patients whose psychosis was of short duration. This

point is interesting since it shows more than ever the need of careful control groups. Accounting for spontaneous remissions in younger patients with short psychotic histories has been the bane of investigators in the field of psychiatric research for many years.

Fully aware of the fallacies of nosological classification, the authors make a few tentative statements about the type of illness which responds to psychosurgery. These are of interest, particularly if one considers that lobotomy has been recommended in anxiety neurosis. Although no psychoneurotic patients were included in this sample, the authors of this section state, "One should be hesitant to recommend operation for only but the most severe neurotics and then only if intensive and skillful psychotherapy has proved valueless" (p. 428). They feel that only schizophrenics with affect somewhat appropriate to ideation should be used as subjects. This obviously includes the better integrated schizophrenic with whom, naturally, results would be more striking. They feel that a manic-depressive in the depressed phase is a good candidate. They do not have evidence on psychopaths but feel that tension in this type of personality is not of the same kind as that relieved in psychotic patients. They do not feel that surgery can replace EST as a treatment of choice. Lastly, the improvement resulting from operative procedure is basically due to a change in affective response, and no patient was actually worse after the operation. Some cases showed temporary improvement only to have a relapse.

We see then that the effects of the removal of the frontal lobe tissue are not definitely established, although the Greystone study has added immeasurably to our knowledge of this problem and has uncovered new problems. The reviewer believes that improvement of the control group by use of pseudo-lobotomy and "total-push" therapy, including individual and group therapy, will yield additional knowledge concerning the effects of lobotomy. The Greystone study does show that selection of patients for lobotomy is still a difficult problem and that it should not be taken as lightly as it has been in the past. "The psychiatrists' therapeutic armamentarium," to quote Heath, still includes intensive therapy.

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